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HABIT FORMATION AND HIGHER MENTAL PROCESSES IN ANIMALS

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This review covers primarily the years 1921-1925, but certain earlier and later dates will be included when completeness in the treatment of a particular topic demands it. The last reviews on the general topic were one by Shepard in 1920 (113), and one on habit formation alone by Maupin in 1921 (87). We shall subdivide the field for review as follows:

1. The formation of simple sensory habits (fish).
2. Discrimination box, maze, problem box, and multiple choice box habits, further divided into:
 - (a) The ability of song birds to form these habits.
 - (b) The influence of drugs and endocrine substances upon learning and retention (rats).
 - (c) The results of cerebral extirpation upon learning and retention (rats and monkeys).
 - (d) The reliability of maze and problem box as measures of individual performance (rats and chickens).
 - (e) Interference and transfer (rats).
 - (f) The effect of different time intervals between stimuli; of different time intervals between learning and relearning; and of different distributions of practice upon learning (rats).

- (g) The effect of variations in the internal variables involved in maze and problem box: to wit, strength of drive, shape of maze, and duration of preliminary feeding (rats).
- (h) The qualitative aspects of maze errors (rats).
- (i) The ability to discriminate between an inclined path and a horizontal path (rats).
- 3. Initial exploratory impulses (rats, chickens, monkeys).
- 4. Individual differences and their inheritance (rats, chickens, mice, apes).
- 5. Experiments indicating that animals discriminate on a relative rather than on an absolute basis (chickens, guinea pigs, apes).
- 6. Experiments indicating that animals respond to objects and not to pure qualities (chickens, apes).
- 7. Experiments designed primarily to investigate higher processes.
 - (a) Delayed reaction (cats, primates).
 - (b) "Insight" experiments (dogs, primates).
 - (c) Imitation (dogs, primates).
 - (d) Investigations of "speech" and of response to symbols (dogs, primates).
- 8. Theory.

1. *Formation of simple sensory habits (fish).* Westerfield (130) and McDonald (79) taught minnows to discriminate food tones and to respond either by leaping out of the water or coming to the top of the aquarium. Other cues were controlled.

Buytendijk and Remmers (14), Burkamp (9), von Frisch (32), Kühn (65), Schiemenz (111), and Wolff (134) all have taught fish under quite varying conditions to respond to certain colors and not to others as indicators of food.

2. *Discrimination box, maze, problem box, and multiple choice box habits.*

(a) *The ability of song birds to form these habits.* Buytendijk (11) taught siskins to discriminate a "food" door by its position in a series of four doors. He obtained evidence for retention after an interval of three months.

Schut (112) repeated Buytendijk's experiment with other species, *i.e.*, goldfinches, birds of paradise, weaver birds, etc. Schut also taught siskins to discriminate the food door when it was distinguished only by its color and varied in position.

Sadovinkova (108) taught canaries, siskins, bullfinches, chaffinches,

pine grosbills, crossbills, tomtits, parrots, and finches to run a Hampton Court maze. Changing the color, texture and odor of the maze by fresh paint, did not upset the habit. Turning the maze by 90° or 180° did. But after repeated turnings the habit ceased to be upset. Sadvinkova (109) also worked the multiple choice (nine door) apparatus. One canary solved three problems: "first door left," "first door right," and "second door left." A bullfinch solved "alternately first door left and first door right." A siskin learned three problems: (1) first door to the left; (2) middle door; and (3) second door from the left. In the latter problem the so-called "ideation curve" was obtained. This bird was then taught further to give solution No. 1 if no paper was present on the floor of the choice compartment, solution No. 2 if paper was present on the floor, and solution No. 3 if a black ribbon was present.

(b) *The influence of drugs and endocrine substances upon learning and retention (rats).* MacDowell (81), and MacDowell and Vicari (83) examined the effect of daily saturation with alcohol vapor upon maze ability (Watson maze) and multiple choice ability (Problem No. 1) in rats. The test animals took more time in learning and retention than did the controls. The untreated children of the treated animals, when compared with second generation controls, also showed up more poorly in the maze and in the Yerkes multiple choice box, Problem No. 1. The untreated grandchildren of the treated animals, when tested in the maze, similarly showed up more poorly in both time and errors in learning and retention than did their controls.

Macht and Seago (86) investigated the effect of ovariectomy upon the maze behavior of female rats. The operated animals were much slower in maze running than were their controls. For both normal and ovariectomized rats the effect of corpus luteum injections was to decrease both running time and errors, both on the day of the injection and the following day. Injections were made both intramuscularly and subcutaneously.

Blatz and Heron (8) examined the feeding of dried pituitary extract, dried adrenal cortex, and dried brain tissue (control) upon performance in the Carr maze and the inclined plane box. The pituitary-fed animals did best, the control (brain tissue-fed) animals were intermediate, and the adrenal cortex-fed animals were poorest. The children of the three groups were fed in corresponding manners. The adrenal-fed children of adrenal-fed parents were again poorer

than the brain-fed children of brain-fed parents. The pituitary group had by this time become too small for the results to be conclusive.

Rickey (105) tested the effects of thyroidectomy, of thyroid gland injections, and of thyroid gland feeding upon behavior in a circular maze. Thyroidectomy caused loss of the maze habit and prevented the acquiring of the habit in animals not previously trained. Injection of the thyroid gland substance caused such hyperactivity that learning became impossible. Feeding of thyroid substance caused loss of skill and inability to form new habits. The children of parents who had been fed or injected during the breeding season showed inability to learn. The children of thyroidectomized females showed inability to learn, but, when then fed with thyroid gland substance, these latter rapidly improved their performance. Grandchildren of thyroidectomized females showed no learning until injected with thyroid gland substance and then improved rapidly.

The above results, in so far as they are statistically reliable, indicate (1) that *alcohol* decreases ability of the animals themselves and of their children and grandchildren; (2) that *ovariectomy* causes complete slowing down; but that (3) *corpus luteum injections* speed up ability again; that (4) *pituitary feeding* helps learning ability; that (5) *adrenal feeding* decreases it; that (6) *thyroidectomy* causes loss of habit and inability to learn; that (7) feeding or injecting *thyroid substance* causes such hyperactivity as to prevent learning, save in the children and grandchildren of thyroidectomized females.

(c) *The results of cerebral extirpation upon learning and retention.* Lashley presents a series of fundamental investigations.

(68) First, he destroyed different parts of the rat's cortex and found that after destruction of any given part a rat could still acquire at normal rate both a simple maze habit and a visual discrimination habit. He discovered further, however, that if, after the visual discrimination had first been acquired, he then destroyed the occipital poles, the habit went. It could, however, then be reacquired at normal rate. The conclusion is that with intact occipital poles the visual discrimination habit normally became localized in them, but it can, if they are absent, be equally well localized elsewhere.

(69) Second, he overtrained the visual discrimination habit of rats by 1,200 trials, and then destroyed the occipital region to see if such an overtrained habit had become transferred to lower centers, as some authors have assumed. The destruction of the visual region caused the loss of the overtrained habit. Hence the hypothesis of a transfer to lower centers must apparently be abandoned.

(70) Third, he investigated the effect of destroying both the stimuable areas of the motor cortex and the underlying cordate nuclei of the rat upon the visual discrimination habit. His results indicated that neither the stimuable motor areas nor the cordate nuclei are necessary either for the formation of or the retention of a visual discrimination habit. He concludes that the function of the stimuable area and the cordate nucleus must be rather that of governing postural tonus than that of mediating habit associations *per se*.

(71) Fourth, he first destroyed the occipital areas in a number of animals and then trained them after operation in the visual discrimination habit. Then in a second operation he explored the remaining parts of the cortex (different parts for different animals) to discover, if possible, what parts of the cerebrum had vicariously taken over the visual function. He explored the frontal regions, the temporal, the parietal, and parietal temporal regions. All groups gave decided evidence of some retention of the visual habit after the second operation. In other words, the nearly complete exploration of the cerebrum failed to reveal any specific portion which had completely assumed the visual function. He rejects the possibilities that the visual function had been assumed by areas left unexplored by the second operation, or that the habit had been learned at subcortical levels. And he concludes that there is probably a diffuse functioning of the cortex such that the various parts are equally potential in taking over the habit. That is, he assumes that the functions of the destroyed occipital region are taken over by scattered fibers throughout all the remaining parts of the cortex.

(73) Fifth, he investigated the retention of manipulation habits in three monkeys after destruction of the greater part of the stimuable motor areas of both hemispheres. He discovered a perfect retention of three problem box habits acquired before the operation. Further, he found that when the problem box had been originally learned with the right side, the left being at that time paralyzed, this habit was later shifted to the left side, when the right side had in the meantime been made paralytic. It could also be shifted so that the animal used head and neck to perform the act. He concludes that for the monkeys, as for the rats, the function of the motor areas and the pyramidal tracts is not one of mediating complex learned activities *per se*. He suggests that their function is rather the governing of postural tonus.

In this connection there may be mentioned also his tests upon the stimuable motor areas of a monkey (72). A portion of the pre-

central gyrus of a monkey was mapped by electrical stimulation in four tests extending over a period of eighteen days. In each test, lasting about half an hour, the reactions were almost constant. But comparing tests separated by an interval of days, wide variations were found. Within a given segmental field, as for example the arm area, stimulation of the same point in different tests resulted in widely different movements, and at different times the same movement was obtained from widely separated and shifting areas. The general fields, however, from which movements of the face, arm, and leg segments were elicited, tended to remain constant from test to test; but the borders of these fields tended to shift. Fatiguing the monkey before testing produced no significant change in the results.

(74) Next he taught a rat a visual discrimination habit with the left eye sewed up. He then removed the bandage and sewed up the right eye. The visual habit learned by the right eye transferred perfectly to the left eye. He also cites again the experiment on the monkey just reported, in which a habit learned with the right arm transferred perfectly to the left arm. He concludes that the simple reflex path concept is not tenable.

(75) Finally, he destroyed varied amounts of the occipital region in rats and then trained them in the visual discrimination habit. He found that the original learning was unaffected by the amount of occipital region destroyed before the acquiring of the habit. On the other hand, he found a very high correlation between the amount of destruction of the visual region when made after learning and the loss of the habit as measured by a second retention test. His correlation between extent of lesion and loss of habit was .70 plus. He found, however, no relation between the specific part of the occipital region destroyed and the degree of loss of the habit. He concludes: "No part of the cerebral cortex is better adapted for the formation of any particular habit than is any other. Any anatomically continuous cerebral area may serve the learning function, provided it presents a sufficient mass. This must mean that in a problem situation the effects of stimulation irradiate to all parts of the cortex. As the habit is established there comes into being a definite structural modification having topographical position and capable of destruction by brain injury. The learning process is independent of locus, whereas the mnemonic press or engram has a definite localization."

(d) *The reliability of maze and problem box as measures of individual performance (rats and chickens).* Heron (38), Hunter (47), Hunter and Vance (49), Liggett (78), Tolman (121), and Davis and

Tolman (24) have examined the reliability of mazes and problem boxes either by correlating parts of the same learning curve with each other; by correlating learning with relearning; or by correlating scores in successive apparatuses, maze and problem box, or maze and maze. The correlations obtained have all been low, never more than .60, and averaging about .30. Liggett used chicks, the others rats. Hunter and Vance (49) conclude that mazes and problem boxes can not be used either to differentiate individual animals or to differentiate groups of animals. The latter conclusion, as Carr (16) has pointed out, hardly seems to hold. The fact that the individuals within a group are not distributed consistently with respect to one another does not necessarily mean that their mean performance may not be reliably distinguished from the mean performance of some other group. Thus, to indicate an analogy, the Alpha test does not distribute individual college professors consistently with respect to one another. Nor does it distribute individual feebleminded reliably. It does, however, distinguish reliably between the mean performance of college professors and the mean performance of the feebleminded.

(e) *Interference and transfer (rats)*. Hunter's (46) seems to be the only investigation coming under this head. First, he taught his animals to go to the right for light and to the left for darkness in a T-box. He then built up the reverse habit. He found that the latter was about twice as difficult as the original habit. This interference of the first habit with the second showed itself primarily in the first half of the learning of the second habit. He performed similar reverses with a simple T-maze and a 4-choice circular maze. For both types of maze the interference of first habit upon second was much less than it had been in the case of the visual discrimination experiment.

(f) *The effect of different time intervals between stimuli; of different time intervals between learning and relearning; and of different distributions of practice upon learning (rats)*. Yarrow (136) investigated the effect of different time intervals between two sensory cues when a response originally made to one was to be transferred to the other. His rats were first trained to perform a negative response to an electric shock. That is, they were taught in a T-box to back out of the side they were on and to go out the opposite way whenever they felt the shock. A buzzer was then sounded either simultaneously with, 1 sec. before, 2 sec. before, 4 sec. before, or 6 sec. before the electric shock. The negative response made to the electric shock was thus transferred to the buzzer. He

obtained a regular increase in the number of trials necessary to get this transfer with the increase of the length of the interval. In a second set of experiments, animals who had first acquired the negative response to the buzzer were then further conditioned to a light presented simultaneously with or preceding the buzzer. Similar results were found. The longer the interval, the greater the number of trials necessary to transfer the response from buzzer to light. He also investigated the possibility of transference of the response in a backward direction. That is, the new stimulus was now presented after rather than before the old stimulus. If the two were immediately contiguous, this backward association was little, if any, more difficult than the forward one. When, however, an appreciable interval was introduced between the two stimuli, the backward association became impossible (within the limits of the experiment).

Warden (125), using the Carr maze, investigated the effects of various distributions of trials. He tested the effect of 6-hour, 12-hour, 24-hour, 3-day and 5-day intervals with 1 trial, 3 trials, and 5 trials between intervals. For all arrangements the 12-hour interval was best. He also tried with 3 trials a day the effect of 3 hours between trials vs. no intervals between trials; and with 5 trials a day 1-hour intervals between trials vs. no intervals between trials. The introduction of 3-hour intervals gave no improvement over the case of the three immediately consecutive trials per day. The introduction of the 1-hour intervals between trials, on the other hand, was better than the case of 5 immediately successive trials.

Tsai (123) investigated the effects of variations in the interval between learning and relearning. He used both the Carr maze and the inclined plane problem box. For the maze he investigated intervals of 1, 2, 3, 4, 6, 8, and 12 weeks. For the inclined plane box he investigated intervals of 2, 4, 6, and 8 weeks. For both cases he found a linear forgetting curve; that is, the amount of forgetting always stayed directly proportional to the length of the interval. (For human subjects, on the other hand, using a stylus maze, he found the more usual negatively accelerated forgetting curve.)

(g) *The effect of variations in the internal variables involved in maze and problem box: to wit, strength of drive, shape of maze, and duration of preliminary feeding (rats). Strength of drive.* Kuo (67) compared the relative effects upon learning of electric shock, confinement for thirty seconds, a long path to food, and a short path to food. He used a simple four-choice box. He found that the electric shock box was eliminated first, the confinement box second, and the long

path box third, so that finally the short path box alone remained. The dropping out of the undesirable choices occurred suddenly rather than gradually. His results indicate that the order of elimination of wrong responses was not dependent upon the relative frequencies with which those errors had previously been made.

Moss (89) devised a box for measuring the strength of drives. The animals were introduced into an end compartment; the middle compartment contained an electric plate; and the farther end compartment contained a reward of some sort. The strength of the animals' drive toward the given reward was measured by the strength of voltage which they would be willing to cross in order to reach the reward. The method is of interest in connection with maze and problem box experiments, since it suggests a means by which the strength of drives can be measured independently. Moss also used a simple maze and found that food and sex were about equally strong motives in causing learning. Both these motives induced quicker learning than did escape from crushed ice spread along alleys, used as a motive.

Dashiell (22) describes a large checkerboard of interconnecting alleys into which a rat can be placed and the differences in exploratoriness between the hungry rat and the non-hungry rat be quantitatively measured and demonstrated to a class.

Sams and Tolman (110) gave their rats a choice between two spatially symmetrical but temporally different paths to food. Each route to food (one on the right, the other on the left) contained a confinement chamber in which the animal could be delayed for any desired length of time. It was discovered that if the difference in delay was made sufficiently great, say four minutes on one side and one minute on the other, the animals all learned finally to take the side upon which the delay was shorter.

Simmons (115) compared the effects of various motives upon maze learning. In general, she found the order of effectiveness in inducing learning about as follows: (1) female in heat, (2) bread and milk plus return to home cage, (3) litter, (4) bread and milk, (5) sunflower seed, (6) return to home cage, and (7) mere escape from the maze. In the case of the female in heat, bread and milk and return home, and litter, she used a shorter and simplified Carr maze. In the case of the other incentives she used also the standard sized Carr maze. She also obtained evidence (based, however, on only a small amount of data) that animals run with only escape from the maze as incentive for the first part of their practice and then later

given a stronger incentive, showed upon the introduction of the new incentive a very pronounced sudden learning, suggesting that some sort of a *latent learning* had been going on during the weak incentive which made itself known when the stronger motive was introduced. Szymanski (118) has reported similar results as to latent learning. See Simmons' and also Shepard's review for a more thorough summary of all Szymanski's results.

Tsai (124) presented male rats in a simple two-way maze with a female in heat on the one side and food on the other. He obtained a much greater proportion of choices for food than for sex, and this proportion increased with repetition.

Buytendijk (12) taught fresh water snails, when placed upside down on a glass plate, to right themselves. And he discovered that if, after they had turned over, they were then rudely torn off and thrown into the aquarium, they showed no learning in speed of turning. When, on the other hand, the whole plate was dipped into the aquarium so that the animals could slide off into the water in a normal fashion and attach themselves to aquatic plants, they showed a decided learning curve.

Summing up, two conclusions can be drawn. (1) Some drives (rewards) act to cause quicker learning in the same situation than do others (Simmons, Szymanski, Buytendijk). (2) When an animal is presented either with alternative paths which provide separate rewards of different attractiveness, or with alternative paths which in themselves have different desirability (differences of spatial or temporal length to the same reward), the paths will be eliminated in the order of their undesirabilities (Kuo, Sams, De Camp). Thus the Law of Effect in some sense certainly holds, and can not be dispensed with in the simple fashion originally proposed by Watson (128).

Maze-Pattern. Peterson (93), it will be remembered, found that in a maze containing both long and short blinds the short blinds were eliminated first. White and Tolman (130) used different kinds of blinds, *i.e.*, ones with elbows so that both "looked" the same from the entrance, and found that the long blinds were eliminated first. Peterson (95) attacked these results of White and Tolman. Finally, the reviewer would here report that he has since tried to substantiate White and Tolman's results, but without success. But neither has he found that in the White and Tolman type maze the short blind is eliminated first, as it was for Peterson. Obviously more work needs to be done.

Warden (126) classifies the blinds in a Carr maze into three types according to their placing and way of debouching from the true path. He finds that the three types show a distinctly different order of elimination. He does not find any evidence for an earlier elimination *per se* of the blinds nearer the food box.

Under this general head of maze-pattern we may cite again the finding of Sams and Tolman (110) that a "temporally" longer true path is eliminated before a temporally shorter true path. And we may recall and cite also the original work of De Camp (25) that a spatially longer true path is eliminated before a spatially shorter true path.

To sum up, it appears (1) that the characters of the blinds, their lengths, their directions and their ways of debouching from the true path, etc., determine the order of their being learned. It also appears (2) that spatially or temporally shorter paths are distinguishable from and selected as against spatially or temporally longer ones.

Bringing the two sets of facts, those as to drives and those as to maze-patterns, together into a single picture, it appears evident to the reviewer that maze-learning has to be envisaged as really problem-solving in character. The learning of a maze is the discovering of the shortest (De Camp) and quickest (Sams and Tolman) (and perhaps also easiest) route to a "wanted" end situation. The animal, by virtue of his discrimination and manipulation capacities, starts with a certain initial envisagement of the maze, out of which arises his set of initial exploratory impulses. It is the strength of his demands (drive) plus his capacities for discrimination and memory (*i.e.*, his ability to discriminate short from long paths, to recognize blinds, etc.) which act as causal determiners to bring about a modification and improvement of this initial envisagement, together with a consequent final selection of the true path only.

(h) *The qualitative aspects of maze errors (rats)*. Peterson (93) found, it will be remembered, that blinds are progressively entered to repeatedly shallower distances. At the beginning of learning they are entered completely, later they are entered only part way, and finally the rat merely hesitates and does not enter them at all. White and Tolman (130) confirmed this result, and Liddell (76, 77) notes it for sheep in a simple maze.

Here also we may recall Dashiell's (21) finding that blinds pointing in a forward direction are initially entered more frequently than those pointing at right angles from the true path.

Warden (125) divided the errors into two classes: (a) entrances

into blinds, and (b) retracings on the true paths or enterings into blinds in the course of such retracings. And he discovered that it was only those of the (a) type which give a normal learning curve. Those of the (b) type, (*i.e.*, retracings) occur in the first part of learning and then drop out relatively suddenly. He concludes that they represent primarily the initial emotional complex of fear and curiosity and are not indicators of a knowledge of the maze *per se*. Macdowell and Vicari (83) also found that when they divided errors into the two groups of (a) entering blinds, and (b) retracings, it was errors of the (a) type only which measured learning.

Warden (127) investigated the effects of different amounts of preliminary feeding upon the learning of the inclined plane box. A group that had been fed for seven days in the apparatus preliminary to running learned much more quickly than did a group that had had no such preliminary feeding. It would undoubtedly seem that the effect of this preliminary feeding was that of reducing the emotional curiosity complex which otherwise tends to constitute at first a distracting and delaying element.

Koch (56), with a Carr maze, tried the effect of forced training in which glass partitions barred the blinds. She found that training with these glass partitions was very efficacious for later learning, although she never obtained complete learning from the forced training trials alone. Some practice with the blinds open was also necessary.

Alonzo (2) in a simpler maze also tried the effect of forced training. But instead of glass partitions he prevented entrances into blinds by means of a collar and leash. He also found that forced training was very efficacious. Indeed, in some of his cases the forced training by itself brought about complete learning. Alonzo feels that this superiority of his forced training as compared with Koch's was due to the fact that in his case the animals made actual attempts to enter the blinds which were, so to speak, punished by the leash.

Summing up, the reviewer feels that these more qualitative results also reënforce the conception of maze and problem box as cases of problem-solving. The progressive shortening of the entrances into blinds suggests that the cue for the "badness" of the blind comes sooner or later. That the ceasing to enter a blind is not merely a conditioned reflex appears from the fact that a non-hungry rat, even when he has learned a maze, will again enter blinds in spite of the fact that they must still present him with their distinctive cues (Szymanski (119)). Again Dashiell's finding as regards the

relative initial attractivenesses of blinds of different types may be reworded as but a description of the types and strengths of exploratory propensities, as these are dependent upon differences in initial discrimination and manipulation features. Further, the fact that retracings are a different sort of variable from blind entrances again brings out the notion that the maze is a problem and that it involves both emotional and cognitive adjustments. And, finally, the findings of Alonzo, as compared with those of Koch, which indicates that when the animal can actually initiate a wrong response, he learns more than when he is not tempted to make that wrong response at all, indicate still further that learning is a problem-solving adventure.

(i) *The ability to discriminate between an inclined path and a horizontal path (rats).* Dashiell and Helms (23) attempted, in what amounted to a form of discrimination box, to teach rats to distinguish an upward inclined alley from a horizontal or a downward inclined alley. They obtained only a slight indication of learning. It would appear that kinaesthetic cues *per se* are difficult for the rat to depend on alone. And this, of course, fits in with Hunter's earlier results on his so-called temporal maze.

3. *Initial exploratory impulses.* Révész (96, 97, 98, 99, 100, 104), in working with monkeys, young children, and chickens, notes the importance of the kinds of initial exploratory tendencies at the command of the organism as determining factors in the success or failure of learning. Thus he finds in multiple choice experiments tendencies with both monkeys and young children of (1) beginning with an end item, and of (2) progressing in serial order down a row of items. He notes also a tendency towards merely regional localization; that is, the animals tend to react to particular regions, not to particular boxes. Chickens, he finds, exhibit a tendency to peck first at the denser groups of grains. Chickens also exhibit the tendencies of beginning at an end item and of progressing in serial order.

Buytendijk (5), with a multiple choice set-up, notes in monkeys a tendency towards repetition of a just preceding choice.

Atkins and Dashiell (3), as a by-product of the unsuccessful attempt to teach rats to respond to the door last lighted of three doors, noted first a consistent tendency after a first choice to then take the door next to the right or to the left (depending upon the individual animal), and then to progress regularly down the series.

Finally, we may again recall Hamilton's (36, 37) similar findings with his original four box nonsolvable multiple choice apparatus.

4. *Individual differences and their inheritance (rats, chickens, mice, apes).* Katz and Toll (55) investigated the individual differences as to temperament and problem-solving ability of five hens. The rank order of success remained surprisingly constant throughout a wide variety of tests. Tolman (121) obtained decided evidence in white rats that the children and grandchildren of bright parents, when measured by a maze, are brighter than the children and grandchildren of dull parents. Bagg (4) found evidence that a strain of yellow mice were consistently less bright in a maze and multiple choice test than a strain of white mice. MacDowell (82) found no evidence with white rats of an improvement in the maze of children or grandchildren due to the training of parents and grandparents. Koehler (63) reports consistent differences between the "intelligence" of individual chimpanzees.

It is evident, however, that before much further progress can be made in this direction more reliable measuring instruments (*i.e.*, mazes, etc.) must be invented. (See discussion above under 2 (d).)

5. *Experiments indicating that animals discriminate on a relative rather than on an absolute basis (chickens, guinea pigs, apes).* In the typical experiment coming under this head an animal is first trained on one pair of stimuli, A and B, in which B is, let us say, brighter (or larger) than A, and he is trained positively to B. Next, he is transferred to a second situation in which B is presented with a new stimulus, C, where C is still brighter (or larger) than B. The outcome seems to be that in this new B-C situation he tends to respond positively not to the original B, to which in the preceding A-B situation he has been trained, but rather in the great majority of cases to the new stimulus, C. It appears, in short, that the training in the original A-B situation has induced not a positive response to the absolute value of B, but a response rather to the mere relative value of B as brighter (or larger) than A. It is this relative character which transfers to the new situation.

Koehler (60) first demonstrated this relative type of response for brightnesses and for sizes in both hens and apes.

Jaensch (50), Katz and Toll (55), and Riekell (106) have all substantiated it for brightnesses in hens. Wissenburgh and Tibout (133) have substantiated it for brightnesses in guinea pigs. Bingham (7) and Riekell (106) have substantiated it for size in hens.

Riekell (106) also discovered that the relative choice as regards brightnesses would hold even when A, B, and C were not gray but were different brightnesses of the same hue. He found the relative

type of response in young children (*i.e.*, under 4), but as the children got older he found more responses to the absolute brightnesses *per se*. He connects this appearance of the absolute type of response with the appearance of adequate verbalization for the colors.

Katz and Révész (54) trained hens positively to noncolored grains and negatively to colored grains (pigmented), when both were distributed on a white background. They were then transferred to a new situation, in which none of the grains was colored but in which a colored light was distributed over both grains and background. In this new situation they pecked at the grains. That is, colored grains on a colored background are the same as noncolored grains on a noncolored background. Again an indication of the relative type of response.

Burkamp (9) taught fish to choose one of a given series of grays under normal lighting. When the total amount of light was lessened, the fish still chose the same gray rather than one of the brighter ones which now was equivalent in absolute brightness to the original one. Again an indication of a relative rather than an absolute type of choice. He also trained his fish to one out of a number of different colors (pigment) under white light, and when the whole illumination was changed to a colored light complementary in color to the original food color, he found that the fish still chose correctly,—again a relative type of discrimination.

Révész (102) trained hens first to the smaller of two circles and then transferred them to pairs of other figures, *i.e.*, squares, etc. They chose the smaller of these new figures. He also transferred them to new figures which differed not in actual size but merely by virtue of an optical illusion (*i.e.*, the Jastrow illusion and the vertical-horizontal illusion). In these cases also the transfer worked, the animals responded to the "apparently" smaller of the new figures.

To sum up, it appears in the great majority of cases that when animals (including young children) are trained positively to one of two stimuli differing in some given dimension, it is the *relative* difference between the two stimuli which the animals learn to respond to and not the absolute character of the one stimulus *per se*. This is considered by the Gestalt psychologists as a demonstration that the animals are reacting to a Gestalt or structure rather than to separate sensation elements.

6. *Experiments indicating that animals respond to objects and not to pure qualities (chickens, apes).* Koehler (59) trained both hens and chimpanzees to choose the darker of two gray papers when

both were equally illuminated. He then cast more illumination upon the "objectively" darker of the two, enough more in fact so that then it reflected more light to the eye than did the "objectively" lighter paper. The animals, however, continued to choose the "objectively" darker, that is, compensate for this difference in illumination. (N. B.—A sensory cue for such a difference in illumination was present in the fact that the two illuminations extended over on to a common background.)

In another experiment Koehler (59) trained both hens and chimpanzees to choose the larger of two rectangular surfaces when both were placed at the same distance from the animal. He then placed them at different distances. But the animals continued to choose the objectively larger one in spite of the fact that in some cases it was made to cast a much smaller retinal image than did the objectively smaller surface.

In other words, animals, just like most human beings, tend for the most part (except under conditions of special training) to see and to respond to the consistent qualities of objects rather than to momentary sense-qualities *per se*.

7. *Experiments designed primarily to investigate higher processes.*

(a) *Delayed reaction.* Koehler (62) points out that it is possible to carry out the delayed reaction experiment (with higher animals) in a much more direct way than that originally invented by Hunter. Instead of having to introduce a long discrimination experiment in which the animal by mere trial and error has first to discover the relationship between the cue signal (*e.g.*, the position of a light) and the to-be-recalled location of the goal, the connection between cue signal and goal can be directly shown to the animal. For example, with chimpanzees one can hide the fruit before their eyes, or one can place it in a specifically distinguishable container. The delay may then be immediately introduced and the animal has simply to recall the container in which he saw the objective put (or the spot at which he saw it buried). The practical advantage of this method is that it cuts out the time usually spent in the long preliminary discrimination experiment. (For, strangely enough, Koehler discovered that this preliminary discrimination training in its usual form takes just about as long with chimpanzees as with lower animals.) With the chimpanzees he obtained delays of over night, and this also not only in the case where the food itself was hidden (63), but also in cases where simply a tool necessary for reaching the food was hidden (64).

It may be recalled that Hunter (44) originally used this direct method of delay in working with a small child, although he did not emphasize the difference between it and his standard method. He let the child see him hide a toy in one of three boxes. The child, after a delay during which he was disoriented, then had to remember in which box the toy had been put.

Cowan (20) used something very like the direct method of delay with a cat. The experimenter appeared with meat at one of two doors facing the sofa on which the cat was restrained. The experimenter retired into the kitchen behind. The cat upon release had to go out the door in which it had seen the experimenter appear and disappear. Cowan obtained successful delays up to 30 seconds. The percentage of correct responses, although all above 50 per cent, decreased regularly in experiments with a 10 seconds' delay down to ones with a 30 seconds' delay.

Buytendijk (10) used the direct method for testing delay in a monkey. He let the monkey see him place food under an inverted flower pot. As soon as released, the animal went to and overturned the pot. In one instance when he placed a second pot under the first without the monkey's having observed it, the latter upon tipping over the first pot registered astonishment when he found the smaller pot underneath it. In other experiments Buytendijk simply threw a number of pieces of food in various parts of the room. The animal in retrieving one bit "retained" without looking at them memories for the locations of the other bits.

Révész (102) attempted a direct method of delay with fowls without success. He scattered grain, covering it with a very shallow layer of earth, while the birds were watching. As soon, however, as the grains were covered, the birds ceased to peck or to scratch for them. With monkeys, on the other hand, he got successful delay when pieces of food were wrapped up in paper or hidden in boxes.

Ladyginkohts, as reported by Yerkes and Petrunkevitch (139), also used a direct method. Her total problem, however, was more complicated. She first taught her young chimpanzee to match colors. She would hand him one color and then require him to pick out of an array of other colors upon a table the one corresponding to that which he held in his hand. After he had learned to do this, she found that it was possible for her, after having given him this sample color, to take it away and introduce a delay of as much as 15 seconds before allowing him to match it.

(b) "*Insight*" experiments (dogs). De Jong (26) tested dogs

in puzzle box experiments similar to Thorndike's. After a dog had learned to open the box by a lever, the box was then turned to 90° and the dog went to the old corner where the lever had been rather than to the new corner where it then was. Only after many turnings of the box did the animal come to react to the lever *per se*. In an inclined plane box experiment the dog was first taught to open the box with the plane in a horizontal position, but when the plane was then put in a vertical position the dog did not recognize it. De Jong also repeated Hobhouse's experiment of putting meat on a piece of cardboard on the edge of a table with a string hanging down. Contrary to Hobhouse's finding, the dog never showed any evidence of "comprehending" the situation.

Buytendijk and Hage (13) taught a dog to choose that one of a row of doors which had a white circle upon it. He found that after the choice of the correct door had been acquired, a shifting of this cue color to other doors was unnoticed. When, however, the whole box was rotated through 180° , the visual cue was apparently again thereby emphasized and the animal chose correctly.

Monkeys. Buytendijk (10) was not able to teach his monkey to use a stick to rake in food, unless the rake was already placed in the proper position. And the animal did not learn to push food out of a tube with a stick. Buytendijk, however, emphasizes the disinterested curiosity and the use of the eyes in disinterested examining of the environment.

Bierens de Haan (5) taught a monkey to associate food with a particular color pasted over the food door. But the animal took over 100 trials to establish the association. He (6) also trained the monkey to discriminate between a card containing two dots and one containing merely one dot of elliptical shape. As the two dots were placed nearer and nearer together, the animal continued to discriminate them from the elliptical shaped smaller dot until the two dots actually overlapped one another. He emphasizes the part probably played by the animal's visual "Gestalt" propensities, which are presumably different from ours, in determining the animal's relative successes and failures.

Révész (98, 99) was unable to teach a monkey to solve the multiple choice problem of alternately left end and right end of a series of boxes, even when the series had been reduced to only two. This author (104) also investigated abstraction. He employed a four-box multiple choice apparatus. The monkeys were first trained to one of the four different forms: square, triangle, circle and

trapezoid, each having a different color. The animals were then transferred to a new setting in which one of the four new figures was identical in form with the originally correct figure and one was identical in color with this originally correct figure. In the great majority of cases the animals in this second setting chose according to the form upon which they had been trained in the first setting rather than according to the color of the figure upon which they had been originally trained. Even when the animal had been especially trained to color in a first experiment, he had a tendency to switch the criterion to form in further experiments. The experimenter discovered, however, that if a monkey had been trained to an equilateral triangle and was then shifted to a situation in which irregular triangles were presented, he did not recognize the identity.

Apes. As already reported, Ladyginkohts (139) was able to teach her young chimpanzee to match a sample color with one out of a number of other colors. She also was able to teach him to sort colors into boxes similarly colored.

Shepherd (114) observed a male chimpanzee and a male orang, seven and four years old respectively, in the National Zoölogical Gardens, Washington, D. C. The animals failed to use a rake from imitating the experimenter. They did, however, successfully pull in a board upon the far end of which food was placed. And they pulled in a stick upon which food was impaled.

Yerkes (138) reports interesting observations on the behavior of two young chimpanzees which he observed under intimate conditions for some months. The one of the animals subjected to formal tests was able to reach a banana by various methods of swinging on a rope. The animal did not seem to show "insight" as regards a banana in a bottle when the neck of the latter was too small to allow the animal's hand to enter. This animal did solve the box stacking experiment with two boxes to reach food hung from the ceiling. It did not solve the problem of pushing food out of a box with a stick.

Yerkes (137) also reports in detail much interesting observation of the behavior of monkeys and apes (mostly chimpanzees) kept in captivity on the estate of Mme. Arbren in Cuba. No experimental tests of problem solving or habit formation are reported, but the general observations of the daily activities of the animals are highly suggestive.

Koehler in his now classical book, *The Mentality of Apes* (63), reports a series of very illuminating problem-solving experiments which he performed on chimpanzees at the Anthropoid Station in

Tenerife during the years 1913-1917. The general methodological principle which he kept in mind in the devising of the experiments was what he called the "roundabout route" (Umweg). In every case the goal (*i.e.*, food) was placed in some position such that the immediate straightaway passage to it was prevented. The animal, in order to reach the food, had to take a "roundabout" route. In the simpler experiments this detour was merely spatial: the food was placed on the other side of a fence and the animal, in order to reach it, had to make detour around an angle of the fence. In the more complicated problems the detour was no longer literally a spatial one, but involved the use and even the manufacture of tools. A rather telescoped list of the successfully solved detours is as follows: (1) simple spatial detour; (2) pulling food into cage by means of attached string; (3) using sticks to rake in food; (4) using boxes, ladders, one another, and even the experimenter to climb on to reach food hung from above; (5) swinging on rope to reach food; (6) removing a box which interfered with reaching for food outside the cage; (7) breaking a branch off a dead tree to use as a stick; (8) emptying stones out of a box in order to use the latter to reach food; (9) making a long stick out of two shorter bamboo tubes (by fitting end of one into end of the other); (10) using a short stick to pull in a long stick in order to use the long stick to reach food; (11) using a box to reach a stick in order to use the stick to pull in food; (12) emptying stones out of box to use box to reach hanging stick to use stick to reach food outside the cage; (13) pushing food by means of stick out of a shallow drawer lying on ground outside of cage; (14) pushing food by stick from one side of a box, where wire mesh was too close to allow hand to enter it, to opposite side of box where mesh allowed introduction of hand; (15) swinging out on the top of a door to reach food hung from the ceiling; (16) unwinding two coils of rope in order to swing for food.

Koehler finds at least five outstanding features in the characters of the true solutions. 1. The correct solution often appears suddenly and "with a jerk" after a period of delay in which no overt advance towards it has been made. 2. The enhancement of the desirability of the objective by substituting a preferred type of fruit or by adding another piece is often effective in causing a solution when one has previously shown no sign of appearing. 3. The first steps constituting such a solution often necessarily run in a direction directly opposite to that of the final goal. 4. The correct solution always has the form of a smooth curve rather than that of a suc-

cession of irregular accidents. 5. A solution once achieved will repeat itself on successive occasions in ways which are functionally alike, though they may be quite different from one another in terms of the specific body-movements or the specific tool objects which they involve. Thus, for example, once the animals had learned to use a box to reach food, they were equally ready to use tables, or even the back of a passer-by, for this same purpose.

Koehler also makes the distinction between "good errors" and "bad errors." Good errors were ones in which the animal, while not actually solving the problem, attempted some response which was appropriate to certain features of the situation (though not to all its features). For example, a box not high enough in itself was pressed against the side of the wall, hoping it would stick. In such a case he suggests that the solution was correct optically though not statically. (In general, he found the apes better in the appreciation of the "optics" [*i.e.*, the simple spatial features] of a situation than in the appreciation of the statics [*i.e.*, the force] of a situation.) Again, in a case where the food was outside and before the animal had learned to make a long stick out of two short ones, he established connection with the food by pushing the one stick by means of the other until the farther one touched the food. This was correct spatially (optically) though not statically. "Bad errors," in contrast, Koehler designates as ones in which a totally wrong response was made, one which could usually be shown to be a hang-over from just preceding experiments. For example, an animal who had been using the box method for reaching food hanging from the ceiling, was observed when fatigued to bring the box to the bars of the cage, although the task had then been changed to that of reaching for food outside.

Koehler decides that the correct solutions (and the "good errors" to lesser degree) exhibited "insight." They are cases of "einsichtiges Lernen." Such solutions do not result from mere trial and error and subsequent "stamping-in," but can be explained only as resulting from a precursive appreciation of the situation. Finally, he notes certain Gestalt implications of insight. Thus in any case of an insight solution it is the response as a whole which alone governs and gives meaning to its parts. The individual steps in the total response will, if taken by themselves, often appear meaningless or even wrong. For they may point in directions quite opposite to that of the final goal. Only when seen in their setting as part of the total smooth response are they intelligible.

(c) *Imitation (dogs, primates)*. Koehler (63) cites instances in which an animal benefits from observing a fellow ape or the experimenter himself solving a problem. He concludes, however, that such cases of "imitation" occur only when the animal is already capable of understanding the problem. He declares that he found no evidences of learning by imitation in this sense of mere blind mimicry by the animals of what they saw performed. He cites, however, many cases (63, 64) where chimpanzees seem to adopt through imitation fashions or modes of play.

Buytendijk (10) did not succeed in getting his monkey to imitate the experimenter in pushing food out of a tube with a stick.

Shepherd (114) did not succeed in getting either of his two apes to imitate him in the use of a rake.

De Jong (26) failed to teach the dog through imitation to get food by pulling a string, or teach him by "putting him through."

Yerkes (138) reports the case of his young chimpanzee imitating the spitting of a small boy.

(d) *Investigations of "speech" and of response to symbols (dogs, primates)*. Gilman (34) during several months of intimate companionship attempted to teach a dog to associate the heard names of objects with the objects themselves. The dog was to select out of a row of objects the particular one named. She had no success. The dog, none the less, did learn during the same time to perform a great many tricks at command. In other words, he established associations between heard sounds and actions to be performed. She concludes, however, that even here it was more the expression of the voice than the actual words which served as the cue.

Yerkes (138) was unable to train his young chimpanzees to imitate a specific sound. The attempt was made to teach the animal to associate this sound with a banana. A banana was presented in some specific manner daily and while a certain specific sound was made by the experimenter. A variety of different methods of presenting the banana was tried, but in no case did the animal show any attempt himself to copy this verbal response or to use it as indicative of bananas.

Learned (138) recorded by means of musical notation the various sounds made by two young chimpanzees in the course of their daily activities. She found that there were characteristic vocalizations made for the different situations: food, eating, the company of persons, and being together alone, etc. It would seem, however, that these verbalizations were more of the nature of emotional expression

than designations for objects *per se*. A very wide range of possible vocalizations was reported, and Yerkes, commenting upon the general situation, concludes that although the chimpanzee possesses a vocal mechanism comparable to that of man, and also a type and degree of intelligence which would enable it to use sounds effectively for purposes of speech, its lack of any tendency to imitate, *i.e.*, to reinstate auditory stimuli, prevents it from acquiring a language in our sense.

Koehler (60) in some sensory discrimination experiments evoked gesture in a chimpanzee. The animal had to obtain food from one of two boxes. In the beginning he was taught to draw in the box chosen with a stick. After a time, however, the animal was ready merely to "point" to the desired box. Such pointing would seem to be a case of a gesture, *i.e.*, a forerunner of speech.

Koehler (61, 63) also found that if he pasted the photograph of a large cluster of bananas on the one box, and that of a large stone of approximately the same dimensions on the other box, the chimpanzee, without specific training on these photographs, at once showed a predominating choice for the box with the banana picture. This, the reviewer thinks, may indicate a response to a picture or symbol, as such.

To sum up, it appears that both dogs and apes can learn readily to understand verbal commands, though neither of them seem able to learn to understand words as symbols for objects. In the ape, none the less, there is some evidence of response to pictures as symbols. (Note also in this connection their use of mirrors [Koehler (63)] and their use of pointing as a means of gesture.)

8. *Theory*. There seem to have been two trends in the pronouncements as to theory: on the one hand, the principle of the conditioned reflex has been made paramount; on the other, less simply mechanical envisagements have been proposed.

(a) *The conditioned reflex*. Cason (19) in his review on the conditioned reflex cites Smith and Guthrie (116), Wilson (131), Hunter (48), Givler (35), Humphrey (39, 40, 41, 42, 43), Woodworth (134), Strong (117), Dunlap (28), Gates (23), Frank (31), Allport (1), Kantor (51, 52), Kuo (66), and himself (17) as all making use of the conditioned reflex to explain learning. McDougall (80), Mursell (90), and Peterson (94), on the other hand, he cites as seeing difficulties with it. The present reviewer also would wish to be classed with this latter group. It is, of course, not the facts of the conditioned reflex which can be criticised, but

rather the tendency to oversimplify these facts. Consider, for example, the salivary reflex itself. The reports from Pawlow's laboratory indicate that for the conditioning to remain for any length of time it is necessary actually to feed the dog practically every time after the response. In other words, the reviewer suggests that this indicates that the dog secretes saliva to the new stimulus, pitch or color, only so long as this actually proves a cue or "symbol" for coming food. But, if this is so, it would appear to be not so much a conditioned reflex as a conditioned meaning. Or, again, consider one of Watson's own descriptions. In speaking of crying, he (129) says: "Crying as such very shortly becomes conditioned. The child quickly learns that it can control the responses of nurse, parents, and attendants by the crying and uses it as a weapon ever thereafter." Now I ask you! What is this but a perfectly bald admission that what the infant has attached to the new situation is not the mere crying reflex *per se*, but crying as a *means to an end*. In short, the reviewer concludes that the law of effect is as important in the conditioned reflex as it is elsewhere in learning. But, if so, then the distinctiveness of the conditioned reflex as a peculiar or fundamental principle disappears.

(b) *Theories proposing less simply mechanical envisagements of learning.* All such less simple theories may be said to start with the importance of the law of effect. And some of them would even go so far as to describe learning in purposive and cognitive, or at any rate pseudo-purposive and pseudo-cognitive terms. Carr's (15) theory belongs, perhaps, in this category in that he emphasizes that the doing of the wrong response is as necessary for the fixing of the right response as is the doing of the right response itself. That is, the animal has to compare and learn the respective merits of the two. Peterson (91, 92) also belongs here. For he has demonstrated the empirical breakdown of frequency and recency of response as the determiners of learning and has substituted his own new principle of the completeness of response. And this, in so far as the reviewer can understand it, seems to imply something like a purposive and cognitive adjustment whereby the maze situation as a whole dominates the responses. Buytendijk (10, 11, 12, 13, 14), Demoll (27), Ettlinger (29, 30), Heymans (39), McDougall (80), Koehler (64), and Koffka (57, 58), the reviewer himself (122), and probably others as well also belong in this group. Instead, however, of attempting here to reproduce and epitomize each such doctrine separately, the

reviewer will permit himself the indulgence of outlining his present final theory as he now sees it.

This theory will start with the proposition that all learning is *problem solving*. For by this characterization of it as problem solving, learning's two most important traits will be emphasized; viz., (1) the fact that it never occurs save under the compulsion of or with reference to ends; and (2) the fact that it is always to be described as an improvement in the organism's adjustment with reference to such ends. The organism starts with certain initial postulations, and learning consists in a change in one of these postulations. Specifically, there are, we shall say, three such initial postulations: (1) a postulation of the *character* of a to-be-sought or a to-be-avoided *end object*; (2) a postulation of the *character* of the immediately *surrounding (means) objects*; and (3) a postulation of the *position* of the *end object* with reference to the immediately surrounding (means) objects. Learning involves the modification of one or more of these three. Thus, for example, Thorndike's cat in the puzzle box starts with three postulations as follows: (1) that of certain food characters (*i.e.*, certain discrimination and manipulation possibilities) in the end object, the food outside; (2) that of certain discriminative and manipulative characters in the surrounding box (means object); and (3) that of a certain position of the food with reference to these discrimination and manipulation possibilities of the box. And the animal's learning consists primarily in a modification of the third, *i.e.*, in the postulation of the position of the food with respect to the box. (In the case of Lloyd Morgan's chick, the learning would consist primarily in a modification of the first (*i.e.*, of the postulation of the character of the end object).

Finally, we must emphasize that each such postulation has a perfectly objective, behavioristic definition. The animal's postulation of the character of the food is found quite empirically and objectively in the fact that the food must actually prove to have certain given physical discrimination and manipulation possibilities if the persisting towards it is to continue on successive occasions. In other words, if it should prove not to present certain actual tastes, chewabilities, etc., the cat would cease to strive towards it. His striving for it implies that these characters, rather than others, are in it. Similarly, his postulation of the character of the surrounding box consists quite empirically and objectively in the fact that the box must actually have certain physically definable discrimination and manipulation possibilities, if the cat is to continue to be ready to release one and the

same set of behaviors with respect to the box. If, in other words, the box should prove not to have certain properties, such as a given outline, resistance, shape, confiningness, etc., the cat would cease to be ready to behave to it in the ways that he is. The ways in which he is ready to behave to the box imply that the box has these certain characters and not others. Finally, his postulation as to the position of the food with reference to the box is found in the perfectly objective fact that the food must actually have certain positions with respect to the box (*e.g.*, outsideness to, nearness^{*} to, possible reachableness by squeezing through, etc.) if the actual selection and ordering of his ready-to-be-released behaviors are to remain the same. If the food should prove to be in some entirely different position with respect to the box, for example, inside it, then the selection and ordering of his actually released responses would be entirely different. The particular selection and ordering of his actually released responses imply, then, a certain position of the food with respect to the box.

To sum up, postulation of a particular character in the end object resides in the dependence of the *persistence aspect* of the behavior upon there proving to be certain actual characters in the end object. Postulation of particular characters in the means object resides in the dependence of all the *ready-to-be-released responses* upon there proving to be certain actual characters in the means object. Finally, postulation of the position of the end object with reference to the means object resides in the dependence of the *selection and ordering of the actually released behaviors* upon the end object's proving to have certain actual positions with reference to the means object.

Let us consider now in more detail the specific mechanisms of learning, both for the type of problem, represented by Thorndike's cat, and for the type represented by Koehler's apes, say the case of learning to use a stick to rake in the food. For both types the learning involves, consists in, an improvement in the postulations as to the position of the goal with respect to the means object. And for both types this learning registers itself in a new selection and ordering of the actually released responses from among the potentially releasable ones. Thus, in the instance of the cat, learning means that the propensity to certain bitings and chewings and clawings has become less, while the propensity to one particular clawing has become increased (*i.e.*, that of clawing at a particular corner or loop). And, similarly, in the instance of the ape, the propensity towards strugglings with the hand between the bars, and the like, becomes less

while that towards the noticing of and use of near-lying sticks becomes greater.

What, then, are the differences between the two types of case? A first type of difference is brought out by transfer experiments. For when after learning we put the cat in other somewhat different boxes, it turns out that his solution was simply "to claw at a particular corner of the box"; while when we try the ape with different arrangements, we find that what he had acquired was a more universal and generalized affair. He is found to have acquired, in other words, not merely the use of a specific stick, but the use of any stick or even a stick substitute, and to have acquired the ability, too, of using them with many skillful adaptations to suit a variety of minor conditions. This difference between the two cases, however great, is nevertheless, it seems to us, one of degree only. The ape's final postulation of the position of the food is of a less limited and of a more ramifying type than that of the cat. It allows more adaptations and substitutions; but it is not fundamentally different in kind. And the word "insight," if used at all, seems as applicable to the final solution of the cat as it is to that of the ape.

There is, however, it is to be noted, a second difference between the two cases. The cat's learning exhibited the so-called trial and error curve, while the ape's exhibits the sudden drop or the so-called ideation curve. What is the significance of this? We have seen that the cat's final postulation of the position of the end reduces to a relatively limited association between a particular stimulus feature and a particular response. Let us now further note that this association is one which must for the cat be discovered and set up through actual trials. For, either because of the innate limitations of the cat, or because of the intrinsic difficulties of the situation (each is relative to the other), the set-up is such as to make no other way of reaching the solution possible. Thus, the problem for the cat is analogous to that for a human being in learning, say, to get out of a maze. In all such cases no precursive insight is possible. For both the man and the cat there must first be trial and error and then selection based upon the results. In the problem for the ape, on the other hand, the set up is such with reference to the animal's capacities that the required new postulation is immanent in elements already present. The necessary data are already perceptually (or memorially) present. In the case of cat or man, the readjustment and new emphasis upon particular responses requires data to be discovered only through tryings out. In the case of the ape, there is

simply a spontaneous "shuttling into place" of elements already present. Both types of problem involve this shuttling into place, or (if you like) re-Gestaltling of the field (discriminative, manipulative and positional). The difference, however, is that for the ape the data for this re-Gestaltling are already present, while for the cat in the box, or for the man in the maze, they must be acquired, in part at least, through further actual experience. It is thus obvious that the facts of retention will play a bigger rôle in the cat's or the man's manner of solution than they will in that of the ape. But for either type, that of cat and man, or that of ape, the final thing is something more than retention. It is a re-Gestaltling (discriminative, manipulative, positional).

So much for the general character of learning as the reviewer now sees it. It will be asked, however, what does such an envisagement of learning contribute? The answer is that it opens up new and re-emphasizes old empirical problems (and that, of course, is all that can be demanded from any theory). In conclusion, then, we may well briefly sketch an outline of these problems.

(1) It is obvious that first of all we need more empirical investigation as to the nature of the initial Gestalten, *i.e.*, the initial discrimination-manipulation and positional postulations. The experiments of Koehler, Jaensch, etc., on grays, sizes, and the like (*i.e.*, experiments on discrimination and manipulation postulations), and the experiments of Révész, Dashiell, Hamilton, etc., on the types of initial exploratory impulses (initial postulations of the position of the end object), and experiments indicating the nature of the sensory cues (visual, kinaesthetic, etc.) upon which these initial postulations can be based, are in point here.

Secondly, it seems that we need experiments to define the nature of the final Gestalten. We want, that is, more discrimination experiments, but we want them after rather than before learning; and in particular we want final transfer experiments to define the final position postulations which have been acquired. It is these transfer experiments which will give us the true definitions of the final solutions.

(3) Thirdly, we want experiments getting at the causal determiners of the re-Gestaltlings, both for the case of trial and error learning and for the case of non trial and error learning. One of the first of such determiners, the importance of which has already been indicated, is the *drive*. Koehler noted its importance in the case of non trial and error solutions, and Simmons, Moss, Szymanski, etc., in

the case of trial and error solutions. As to other determining factors, we are still pretty much in the dark. Koehler has, to be sure, indicated certain ones for the ape problems. Thus, for example, for the case of the stick solution he found the ability to see stick and objective in one and the same *coup d'oeil* had a strongly facilitating effect, while having to pass too near the goal in the progress of an Umweg was distracting and inhibiting. He also discovered some of the general capacities of the ape which made certain types of problem relatively easy and others relatively difficult, for example, the general goodness of their grasp of simple spatial relations (what Koehler designates as the "simple optics" of a situation), and the general poorness of their grasp of the gravitational relations of objects other than their own body (*i.e.*, what he designates as the "statics" of a situation). For the cases of trial and error learning, on the other hand, we are still surprisingly in the dark. We may emphasize, however, such experiments as those of Koch and Alonzo, which, as the reviewer sees them, are attempts to control the retention and experience factors separately, by giving specifically defined preliminary experiences, and thus to discover the nature of the re-Gestaltling *per se* after the necessary re-experiences have been acquired. And we would suggest also the value of experiments upon the effects of maze pattern, *i.e.*, length and direction of blind with reference to the true path, etc. These should give insight into the interrelation of types of discrimination and manipulation postulation and resultant possibilities as to position postulations.

Finally, the probability would seem great that after such further experimental analyses the laws both of Exercise and of Effect, as we now know them, will no longer be tenable or even particularly meaningful. (See in this connection the present criticisms of Cason (18) and Peterson (92).)

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RECENT CONTRIBUTIONS TO THE EXPERIMENTAL LITERATURE ON NATIVE OR CONGENITAL BEHAVIOR

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A recent review of articles on native behavior prepared for this journal (Tolman, 1923 (59)) dealt primarily with the *theories of instinct*. This review will be devoted exclusively to recent experimental studies of native or *fundamentally* native behavior in vertebrates.¹

EMBRYO, FOETUS, AND NEWBORN

During the latter part of the nineteenth century anatomical researches by the morphologists made it possible to epitomize the story of somatic development; from the fertilized eggs to a well developed embryo. More recently, experimentalists, working from these morphological foundations, have so extended the embryological data that they now include, for a limited number of species, detailed analyses of the nature and order of integration of the most primitive units of response.

Responses of the Embryo

Coghill's correlated studies of the anatomical and physiological growth of the amphibian nervous system continue to appear. The earlier papers (11, 22) correlated neuromuscular development with the initial head flexures, coils, and swimming movements of *Amblystoma* larvae. Recent papers (12, 13, 14), in so far as they concern our present interest, deal with the neuromuscular mechanisms by which movements are integrated at various levels of complexity of response. He has succeeded to a remarkable degree in demonstrating the relation between somatic and functional maturation of neuromuscular response mechanisms. Concerning the order of

¹ Page limitation makes it necessary to restrict the review to papers appearing during the past five or six years. It is important to note, however, that much of the experimental data on native behavior for many years preceding 1920 have never been assembled.

development of crude responses as opposed to simple, definite (reflex) movements, he draws the following important conclusions:

"The growth of the nervous system, in so far as it has been definitely correlated with the development of the behavior pattern, demonstrates that *fractional patterns (reflexes) arise by a process of individuation within a primarily integrated total pattern, and that the latter does not arise by an integration of independent reflexes. The form of the behavior pattern in Amblystoma up to and including locomotion is determined by specific neural counterparts that acquire their specificity in functional value through laws of growth in the nervous system. There is evidence also that mechanisms that condition the performance of such a behavior pattern as locomotion in mammals are determined in the same manner.* (Italics mine.) It is important, therefore, to know how far growth, in the sense of the differentiation of new functional parts of cells, is projected into the life history of the vertebrate, for so long as it continues it must participate in the function of the nervous system as a whole, and, therefore, in the development of the behavior pattern." (Coghill, 14.)

Closely related to Coghill's work is Tracy's comprehensive study of the development of motility and behavior reactions in the toadfish (60). Tracy attempts to outline the patterns of body movements at the time of their initial appearance in the embryo and as they change in successive stages of development until the animal becomes free swimming. He finds that the earliest bodily movements arise from internal (endogenous) stimuli acting upon motor nervous pathways while the exteroceptive and proprioceptive nervous systems are still undifferentiated. Rolling the embryo to and fro by pressure upon the egg membrane produces no response. Even at hatching the toadfish does not respond to external stimuli, to passive bending, or coiling of the body, yet spontaneous movements of great variety and vigor occur before and during this time. Motility appears first in the discrete elements of the embryonic muscle complex. At first there are tentative, irregular movements which are superseded gradually by more and more coordinated acts, indicating the gradual development of the mechanisms concerned. These spontaneous movements become movements of progression when the larva is released from the egg membrane and the yolk sac.

While functional development of the exteroceptive and proprioceptive systems is in progress the spontaneous movements continue with little change. They may originate independently in any part of

the primitive motor system as is demonstrated by cutting the spinal cord at various levels.

From the standpoint of priority, reactions involving the receptor portion of the nervous system may be elicited by environmental stimuli according to the following order: Tactile, vibratory, proprioceptive, rotatory, and light. Tactile sensitivity of the skin in various regions of the body appears at different ages. First, the region around the external branchial aperture (operculum and lateral surface of pectoral fin) produce a coil response to the same side as the point of stimulation; second, the posterior third of the body yields a coil response to the side opposite that stimulated; third, the anterior head region (cornea and pre- and post-orbital regions) produces a coil to the opposite side; fourth, the anterior body region gives rise to a coil to the same side. The outer surface of the operculum and the lateral surface of the pectoral fin are considerably more precocious than the remaining so far as yielding the "flutter" or swimming movement which appears later than the coil responses. All of these responses may be elicited while the larva is still attached to the yolk sac and prior to the free swimming stage.

Reactions to rotation appear at a very early age (embryos of 11 mm.). There is a fairly definite temporal order for the appearance of the different rotatory reflexes. First, there occurs a "slow phase of nystagmus during rotation, slow roll of eyes back to resting position on stopping rotation (11 mm.); second, compensatory coil of the body (11.5 mm.); third, back-stroke of the body on stopping rotation, eye comes to rest by a series of oscillations (12 mm.); fourth, quick phase of nystagmus (15.5 mm.); fifth, active swimming opposite to direction of rotation (young adults). The eye responds to a smaller angle of rotation than the body."

Highly adaptive responses and adjustments to changing conditions of life are exemplified by many observations of which the following are typical: "In transition from the attached to the free-swimming existence, the following modifications of responses were observed. To tactile stimulus the threshold for the total reaction (swimming response)² is greatly raised for all areas, and the direction of the initial coil from the anterior body, operculum, and pectoral fin becomes reversed; tactile stimuli also result in an abduction or adduction 'set' of the pectoral fins (beginning of escape reaction) to stimulus applied cephalad or caudal, respectively, to the pectoral

² Words in parentheses mine.

fins. The free-swimming larvae are negatively phototactic and strongly stereotropic and exhibit complicated escape and biting reactions." Observations made upon different species of embryos and larvae lead to the conclusion that the differences in reactions displayed at different stages of their development arise from temporal differences in the establishment of effective connections between various elements of the embryonal nervous system (primitive bilateral motor system, commissural elements, spinal-ganglion cells, Rohn-Beard cells, and receptors).

Carmichael (8) makes a direct and ingenious attack on the problem of maturation apart from the influence of external stimulation. He brought eggs of the frog (*Rana*) and salamander (*Amblystoma punctatum*) into the laboratory to develop under conditions suitable for observation. A number of embryos in their early head and tail bud stages were divided into control and experimental groups. The controls were allowed to develop in tap water, a satisfactory medium for normal growth and activity. The experimental group, on the other hand, was put into a chloretone solution of just sufficient concentration to keep the developing larvae in a state of anaesthesia without injuring the developing organs.

At well known stages in their development (Coghill, 11) the controls began to respond to tactile stimulation along the skin of the body. Shortly afterward the more complex swimming movements might be seen. At the same time, however, stimuli applied to the experimental (anaesthetized) group did not evoke the least response, hence, it may be assumed that bodily movements were totally absent during that period of morphological and functional development in which the larvae normally pass from the non-motile to the free-swimming stage.

After the controls had reached the free-swimming stage, the drugged individuals were transferred from the chloretone solution into a dish of tap water in order that their ability to respond to stimuli of the environment might be observed as the narcosis gradually passed off. Time intervals between the release of larvae into the tap water and their initial responses to tactual stimuli were recorded. Larvae of both species were able to respond to tactual stimulation, on the average, in less than 12 minutes after transference from the chloretone solution. (18 *Amblystoma*; 7 *Rana*). About 30 minutes subsequent to their first responses all of the individuals displayed coördinated swimming movements. "In fact a number of the eighteen *Amblystoma* embryos swam so well in less than one-half

hour after they had shown the first sign of movement, that they could with difficulty, if at all, be distinguished from the members of the control group who had been free swimmers for five days." The experimenter expresses doubt as to whether the gradual perfection of the swimming responses of the denarcotized group during a period of 30 minutes was the result of gradual diffusion of the narcotic from the tissues of the larvae, hence gradual recovery from the anaesthetic, or rapid learning. Later observations³ on the recovery of narcotized controls released simultaneously with the experimental animals into tap water, show the periods of recovery for both groups to be the same. Hence it may be assumed that previous maturation rather than learning in the accepted meaning of that term was responsible for the first swimming movements in the denarcotized animals of his original experimental group.

Foetal Behavior

Avery's report (3) on foetal movements in the guinea pig is the most extensive study of its kind yet reported. The guinea pig because of its long period of gestation (68 days) is very well developed at birth and for that reason particularly well suited to an investigation of intra-uterine development. Foetuses were delivered prematurely from the foetal ages of 45 to 68 days (end of gestation period) and studied under constant laboratory conditions. Although the ages at which they made particular responses varied considerably, very definite developmental trends for specific ages could be detected. Observations on 86 foetuses led to generalizations of the following type: Only a few gasps of breath are observed in foetuses of 45 to 50 days gestation; irregular breathing appears in those of 50 to 60 days; regular and continuous breathing as in the new-born, after the 64th day. On the 45th day, or earlier, a mild electrical stimulus evokes coördinated movements of the limbs and trunk; it evokes breathing gasps even after spontaneous gasps have ceased. By the 48th day, kicking or withdrawal responses may be elicited by pinching the skin of either foot. On the 50th day, a spontaneous kicking movement was observed. Eyelids open between the 55th and 57th days. On the 57th day kicking responses to pin-pricks are noted and likewise eyelid reflexes to air blown on the cornea. Vocal responses were observed on the 58th day; scratch reflexes with the forepaws, on the 59th day; on the 60th, rolling from back to side or over to

³ Personal communication.

haunches, crawling, and twitch of ear to tones and noises. On the 61st day, responses to olfactory stimuli and spontaneous scratch responses appear; on the 63rd, standing and walking; on the 64th, fear and social responses; and on the 65th, sucking. Miscellaneous responses observed were as follows: On the 50th day maxillary movements were recorded; on the 60th day he observed turning and raising of the head, twitching of the leg as the umbilical cord was cut, and urination; on the 64th day, twitch of ear to a tactual stimulus and spontaneous stretching before and negative orientation to an electric heater; on the 65th day, closing of eyes when vibrissae near the orbits were touched; on the 67th day, nosing and chewing shavings, metal, paper, and alfalfa hay without swallowing.

To test the degree to which foetuses of 63 days or older use their ability to orient in utero with respect to gravity, radiograms of the pregnant females held in various postures were made (*e.g.*, belly down, belly up, on right side, and on left side). The results were consistent in showing that the young did not shift their positions as the mother's position was shifted although she was held for various lengths of time in an abnormal position. In one series of radiographs the mother was shifted at five minute intervals during a period of 30 minutes and radiograms made before each shift. The young kept their original positions in the uterine tubes throughout the half hour. When, an hour or so later, they were removed from the mother, all clambered to their feet immediately after delivery and returned to that posture if put upon their sides or backs. Avery concludes tentatively that the animals do not exercise the ability to orient gained before birth although this ability is in a very high degree of maturity at least four or five days before the end of the gestation period. His observations signify that foetal development proceeds simultaneously along functional and structural lines and that the integration of structural elements to form mechanisms for pattern responses is an inherent property of the tissues involved and governed primarily by metabolic processes operating within the individual. Their integration is not primarily conditioned by the play of stimuli of the external environment upon the individual, excepting, of course, those environmental factors underlying normal somatic development.

At the Wistar Institute of Anatomy, Philadelphia, an important study of the correlated growth of the nervous system and foetal behavior in the albino rat was in progress during the past year under the general supervision of Prof. Geo. E. Coghill. As yet only the preliminary report of this study has been accessible. In general the

problem is similar to those pursued by Coghill on the amphibians, but adapted to the more complex mammal under consideration. A novel technical feature of the study is the use of a motion picture camera to record movements of fetuses of various ages. Subsequently slow motion projection is used to analyze the responses.

Minkowski (30, 31), a Swiss neurologist, has the signal honor of publishing the most comprehensive study of precocious movements, reflexes, and muscular reactions of the human foetus so far reported. His materials consisted of 17 living fetuses varying in ages from 2 to 5 months and removed because of medical indications (tuberculosis, cardiac defect, nephritis, epilepsy, etc.). They were delivered by Caesarian section under local anaesthesia and immediately put in a saline bath at 40° C. for observation under standardized conditions. Progressive asphyxiation takes place in fetuses of these ages, hence only observations immediately after removal catch the fetuses at what approximates their highest levels of behavior. Sample observations are as follows:

Fetuses of from 2 to 5 months perform more or less active movements of the head, trunk, and limbs after extraction. The head is turned to the one side or to the other, the trunk is bent and stretched, the limbs are flexed and extended, adducted, or abducted, rotated toward the inside or the outside. These movements are slow, asymmetrical, arrhythmical, and non-coordinated. Thoracic movements appear only in the older fetuses. The foregoing movements persist only a few minutes, sometimes not exceeding a minute and a half.

Stroking the skin with a light brush brings out various reflex movements. These may stop for the time the spontaneous movements or be superimposed upon them. At these ages reflexes are not confined to the parts of the body stimulated, but are irradiated over the whole body. "Thus one may say that every part of the skin can serve as a reflexogenic zone for quite variable reactions which tend to spread more or less over the whole foetal organism."

Touching the lower lip or the tongue with a blunt probe sometimes elicited movements of the lips or the jaws or the tongue. Touching the closed eyelid causes contraction of the orbicularis even before the opening of the palpebral fissure.

Tendon reflexes could be elicited with some difficulty. Smallness of body relations, however, obstructed this work. A percussion in the region of the patellar tendon resulted in contraction of the quadriceps femoris in a foetus of 6.5 mm. (about 2 months of age).

Deep cervical reflexes were elicited by changing the position of the head in relation to the trunk. Movements of the arms oftentimes follow head movements. These are less regular in form in the young than in the older individual.

Vestibular reflexes were elicited by moving the body from one plane to another although care was taken to keep the members of the body immobile. The reactions were extremely variable.

Considering jointly the observations of Minkowski on the foetus, Blanton (6) on the new-born, and Gesell (19) on the infant and pre-school child one now has the necessary data for a bird's eye view of the early developmental process of the child. The latter study, particularly, suggests the important point that *mental growth is in all probability only another aspect of the more overt developmental processes described by Minkowski and Blanton.*

Observations on the New-Born

In the study of Hartman (21) on young opossums we have a description of what appears to be an extra-uterine foetus but one that, strictly speaking, is only a *very young* new-born. *Opossums are delivered after a gestation period of eleven days.* When freed from the foetal membranes they clamber up the hairy abdomen of the mother, crawl into her abdominal pouch, and firmly attach themselves to a nipple where normally they develop for several weeks before detachment. With respect to the young climbing to the mother's pouch Hartman says, "Actual observation, however, showed the process to be as simple as it is remarkable. For the young reach the pouch without the aid of the mother—these eleven-day-old embryos are born into the world with sufficient neuromuscular coördination and sensory response to clamber from the vaginal orifice into the pouch, find the teat in a maze of hair and attach themselves for a two months' stay at this haven of food and shelter. The writer witnessed this migration. The young appear at the genital opening and after being licked free of liquid and embryonic envelopes by the mother they climb 'hand over hand' into the pouch. Because of the position of the mother during parturition the young must climb upward to reach the pouch."

With respect to the new-born and infant guinea pig, Avery says (3): "The guinea pig is unusually well developed at birth. It is covered with hair, eyes and ears are open and the teeth have erupted. It is able to roll from back to side and side to haunches. It can crawl, stand and walk, and execute the scratch reflex. It

responds to pinch, heat or prick with a jump or kick. Electrical stimuli elicit a twitch, respiratory gasp, jump, and cry. Both visual and auditory responses are given to appropriate stimuli. Swallowing and sucking responses may be elicited. Olfactory stimuli evoke movements of the head. No fear is manifested toward man or the white rat but the young guinea pig retreats at loud noises." All of these characteristics and abilities are present at least four days prior to birth and require only appropriate stimuli of the environment to call them out.

Bird (5) has repeated and greatly extended Breed's earlier work on the maturation of feeding responses in chicks. His larger number of subjects and more elaborate system of controls are especially noteworthy. Among the conclusions are the following: "The most rapid increase in accuracy of swallowing grains occurs during the three initial practice days, whether these three days immediately succeed the time of hatching or come after a period of artificial delay. A part of the increase is attributed to general physiological development, which occurs during the first few days of postnatal life, irrespective of practice in pecking. Delayed practice is followed by accuracy of swallowing which at the initial test is no greater than that observed in one day old animals."

Another investigation on accuracy of pecking in chicks was reported by Mosley (32). She verified the data of Breed on the relatively complete development of the pecking response at the time of hatching, but found a decrease in the rate of improvement of the pecking response in chicks fed artificially for various periods of time after hatching. The latter is contrary to Breed's findings. Learning, in the accepted sense, accounts primarily for increased accuracy in seizing objects, whereas striking and swallowing are more nearly unlearned responses.

APPETITES

Although one usually speaks of the feeding habits of adult animals with more exactness than of the native eating responses, it can hardly be denied that the internal factors regulating appetite retain much of their original nature. For this reason I shall briefly touch upon studies of food-intake.

Wang (62, 63) observed the strengths of appetite in rats of different ages and sex and, in the female, during the periods of oestrus, dioestrus, pregnancy and lactation. The average daily food-intake runs parallel for the male and female until the age of 50 days; there-

after the former consumes about one gram more per day than the female. When food-intake is calculated as calories per kilogram of body weight or as calories per square meter of body surface there are two rather distinct phases of food consumption. The first consists of a sharp decrease between the age of weaning and 60 days in the female or 90 days for the male; the second is a slow and gradual decrease for the next six or seven months. During the second phase the food-intake calculated as calories per kilogram of body weight is slightly lower in the male than in the female and is directly correlated with the greater spontaneous activity of the latter.

Wang (63) finds no appreciable increase in food-intake during the period of gestation, but an enormous increase after parturition which continues throughout the period of lactation. When the young are weaned there is a quick return to the normal level of food consumption.

Slonaker (42) finds a low point of food consumption in the female during the peak of activity associated with the receptive phase of activity. The animals were kept in revolving cages. Contrary to Wang (63) he finds greater food consumption in the pregnant females than in non-pregnant females of the same age, except during the last days of gestation when the food-intake shows a very noticeable drop. During the period of lactation the food-intake is roughly proportional to the number and age of suckling young and to the age and activity of the mother.

Richter (40) concludes that rats that have been required to drink alcoholized water (8 to 16 per cent) for long periods of time do not become habituated to this solution. Some ceased to consume the alcoholized water immediately when water was offered, some consumed both pure and alcoholized water, and some continued to drink the alcoholized water with scarcely a taste of the pure water. (To the reviewer it seems that the results are equivocal.) No deleterious effects of the alcoholized water were manifested in body growth but there was a very considerable drop in spontaneous activity.

HIBERNATION

Many theories to explain the mechanism of hibernation have been advanced, but the essential problem is still unsolved. According to Rasmussen (34), lowering of external temperature, depression of respiration by cold, deficient food supply, cerebral anemia, auto-narcosis, acapnia, and altered function of the endocrine system have

all been cited as primary causes, but experiments have shown that not one of these is in itself sufficient to produce hibernation.

Some years ago the lethargic effects of pituitary deficiency were suggested as causal factors in inducing hibernation. Inquiring into the supporting evidence for this theory, Rasmussen (35) made a detailed study of the histological appearance of the hypophysis of the woodchuck sacrificed at every month of the year. Despite the fact that woodchucks undergo severe inanition prior to waking in the spring, the hypophysis does not change in weight or histological appearance as compared with the prehibernating gland. Hypertrophy follows immediately upon wakening in the spring and this hypertrophy is equally distributed among the three principal parts of the gland. In a general way, it would seem, Rasmussen's work supports the findings of Mann (26a) to the effect that the hypophysis has no causal relation to hibernation.

Further studies by Rasmussen (36, 37) on the "hibernating gland" (multilocular adipose tissue) so commonly found in rodents, but not confined to hibernators, lead him to conclude that it is not a secretory gland at all. Although of some importance as a source of food for the hibernating animal, it does not seem to be causally related to the onset of hibernation or awakening after hibernation. Cassidy, Dworkin, and Finney (9) are now working on the theory that a combination of lowered body temperature and low concentration of blood sugar may be responsible for the condition of hibernation. They have shown that some of the characteristic conditions can be brought on in the dog and cat by reduction of blood sugar through injections of insulin into animals whose bodily temperature has been reduced to approximately 20° C. by immersion in cooled water. Low concentration of blood sugar combined with lowered bodily temperature in dogs and cats results in the following conditions commonly observed in hibernating animals:

1. Tendency of the animal to assume the temperature of the surrounding medium.
2. Absence of shivering and of spontaneous movements.
3. Loss of consciousness.
4. Profound reduction in the rate of metabolism.
5. A prolonged state of hypoglycemia without convulsions.

When glucose is injected into the animals the abnormal torpid conditions quickly give way to normal conditions. The authors state that the results obtained so far justify further investigations of similar nature, hence they have begun a series of experiments on typical

hibernating animals in an attempt to induce hibernation and awakening at will by controlling the body temperature and the concentration of blood sugar.

MIGRATION

An important innovation in the study of animal migrations has been made during the past decade. This innovation consists of systematic trapping and marking of migratory animals followed by registration of the marks together with other pertinent facts concerning the species, probable age, physical condition, place of capture and release, etc. If these individuals are captured again, the registered tags are reported to the central station along with data concerning the time and place of capture, condition of animals, etc. Thus a great mass of raw data is gradually accumulating; eventually it will become the scientific foundations for experimental studies of the underlying causes of migration.

Since the United States Department of Agriculture took an active hand in systematizing studies of bird migrations, literally thousands of individual reports have come in from the "banding stations" located in various parts of the western hemisphere. These reports will not only help to establish the facts concerning migratory routes, destinations, stop-overs, etc., but, in addition, will stimulate systematic researches on the causes, orienting cues, and other problems centering about the migratory flight.

Chidester (10) has summarized old and recent publications on fish migrations. This review of the physical and chemical influences on migrations is not only the most recent but also the best available. According to his summary, there is little doubt that schools of salmon remain intact as the "fingerlings" descend the parent stream, while they remain in the brackish waters of the estuary, or roam up and down the Pacific coast. At the age of sexual maturity they again ascend the parent stream to its headwaters and may even return again to the parental spawning beds. The causes of migrations from and to the spawning beds at the beginning and near the end of life are still unknown.

ACTIVITIES ASSOCIATED WITH THE REPRODUCTIVE FUNCTIONS

The recent literature on congenital sex behavior is so extensive that space limitations permit me to review only illustrative studies.

Initial Copulatory Response

Copulatory ability in young laboratory animals has been studied for the purpose of establishing age norms of puberty from which the precocious or delayed appearance of sex activity incurred by special dietary regimes, factors of husbandry, or pathological disturbances of the endocrine glands may be estimated. The most accurate standards now available pertain to the albino rat.

Stone (48) has described the initial copulatory act in the male and determined the age of pubescence (50). Young animals grown under optimum conditions of nutrition and environment manifest copulatory ability at the median age of 47 ± 1.02 days (56 cases). The minimum age for this group was 37 days and the maximum, 72 days. Body weights varied from 90 to 200 grams at the time of first copulation. Nineteen of the 56 males copulated before mature spermatozoa had been liberated in the tubules of the testes and in the majority of cases copulatory ability appeared before the organs of intromission were sufficiently developed to deliver mature spermatozoa into the reproductive tract of the female. (Rats grown under optimum conditions may, as a rule, inseminate females at or about the age of 65 days.

Long and Evans (26) and Stone (48) have described the overt copulatory act in the female rat. When nutritive and environmental conditions are similar, the ages of puberty are about the same for males and females. As reported by Evans and Bishop (16) the mean age of first oestrus is 47.3 days (570 cases). For this group the minimum age was 32 days; the maximum, 94 days. Of special interest to psychologists is the demonstrated fact that in animals as well as man the variability of puberal age is great and in all probability equal in amount to that of other physiological or somatic traits.

Maturation of Sexual Responses Apart from Social Stimuli

The problem of maturation of a postnatal function is approached by Stone (54) in his study of the initial copulatory response of female rats reared in isolation from the age of 20 days to puberty. The receptive stage of oestrus was detected by observing macroscopic signs of oestrus. Twenty individuals were tested. Of these, 19 copulated within a few seconds after being put with a sexually aroused male. The cause for one failure was not ascertained, but a mistaken diagnosis of the receptive phase of oestrus was suggested as the cause. The immediacy of the copulatory response in these

females would seem to justify the conclusion that no factors of environment beyond those necessary for normal somatic development (adequate supply of food, water, suitable temperature, etc.) are required to bring about sexual maturity as manifested by ability to perform the copulatory act during the receptive phase of oestrus. The ages at which initial copulatory acts were performed by these isolated females closely approximated those of other females reared with animals of the same or opposite sex.

Although subsequent tests for periodic receptivity in these isolated females were not made, the point has been adequately covered by Prof. J. R. Slonaker in an adjoining laboratory. It appears regularly in postpubescent females prior to the menopause even though they are isolated from the age of weaning.

Delayed Appearance of Initial Copulatory Act

Evans (16, 17, 18) and his co-workers have shown that the age of first sexual receptivity of female rats can be retarded as to age of appearance, held in a state of dormancy subject to later revival, totally suppressed throughout the lifetime of the individual, or, in other ways, manipulated at will in predictable directions by the effective control of their nutrition. Although similar demonstrations with the males are less extensive, varied, or complete as to detail, they clearly indicate that the same can be done with it. Stone (51) observed that *quantitative* restriction of food in which the day's rations of a well balanced diet were only sufficient to hold the animals at maintenance during periods of 20 days delayed the appearance of the initial copulatory act on the average from 16 to 23 days. Delays of smaller amounts were registered when males were allowed to feed *ad libitum* on a *qualitatively* deficient diet (Stone, 52).

Not only can the awakening of sex activity be manipulated by control of the food-intake, but also by disturbances of some of the internal secretions—the regulators of metabolism. Recently a striking demonstration of this point has been made by Smith (45, 46) working with the pituitary gland. If rats are deprived of this glandular secretion in infancy the congenital sexual responses never develop; if deficiency occurs after puberty, oestral cycles soon disappear and never return unless pituitary secretions are supplied by injecting fresh pituitary glands, or by attaching the pituitariectomized female to a normal female so that the secretion of her pituitary gland is transferred via the blood stream to the deficient individual (experi-

mental parabiosis).⁴ Results of this type cannot be secured in ovariectomized females deprived of the hypophysis, irrespective of the duration of hypophyseal injections. Hence, it may be inferred that the influence of the hypophysis is exerted indirectly and through the medium of the gonad.

Precocious Awakening of Sexual Responses

Descriptions of puberty praecox in which precocious manifestation of sexual reflexes were recorded are handed down to us from antiquity (Pliny). Until recently, however, the data on these comparatively rare individuals were insufficient to warrant valid generalizations concerning the augmentation of "sexual desires," precocious mental growth, and other psychological aspects of the case. From a psychological standpoint the most complete case studies reported are those of Gesell (20). He finds no evidence from his clinical examinations extending over a period of three years in one case and four years in the other to warrant the statement that mental development has been accelerated along with precocious bodily development and early somatic and physiological manifestation of puberty. The affective responses of the individuals studied, however, seem to have been somewhat disturbed and probably precocious as to development. Kulmann and Stone (24) in a recent review of the literature on puberty praecox reached similar conclusions concerning the independence of mental growth and precocious manifestations of puberty. As yet, however, standards are not available for ascertaining whether there were augmented or precocious "sexual desires" in any of the cases reported.

Recent experiments have demonstrated that precocious sexual behavior as well as precocious development of secondary sexual characters may be brought about by injection of gonadal extracts. Allen and Doisy (1, 2) discovered that injections of fluids taken from the mature ovarian follicle of swine will hasten the appearance of first oestrus in immature females, thereby causing precocious manifestation of the "sexual instincts".

Weakening and Suppression of Potency and Sexual Libido Through Castration

The general effects of castration, so far as they affect copulatory ability in man and the lower animals are less accurately established

⁴ I am indebted to Prof. Philip E. Smith, Department of Anatomy, Stanford University, for the privilege of reading the manuscripts of articles now in press.

than are the resulting changes in body structures, particularly the secondary sexual characters. A comprehensive review of recent data on this subject will be found in the book by Lipschütz (25).

When castrated in early infancy, the higher animals studied so far do not become sexually aggressive or sexually receptive at any time thereafter. Castration of males near the age of puberty produces variable effects on sexual aggressiveness, but in all females for which data are available sexual receptivity fails to appear at the normal age of puberty. If postpubescent males are castrated, there is a wide range of variation as to the duration of sexual aggressiveness and ability to copulate, yet it would seem that in all species examined sexual aggressiveness gradually wanes. Stone has completed a quantitative study of the duration of copulatory ability in 45 male rats castrated after puberty (in mss.). Some of the animals when tested copulated for only a few days subsequent to castration, but the majority continued to copulate vigorously from one to three months thereafter. A small per cent of the cases copulated for a period of seven months. Thorek (58) published a similar study on apes. Six apes known to be sexually mature and sexually active were castrated. At the end of four or five months all of them became impotent and sexually inactive.

Restoration of Sexual Aggressiveness and Receptivity

The revival of copulatory ability and sexual receptivity have been repeatedly demonstrated in laboratory animals. Typical of this is the study of Thorek cited above. Some months after the castrated apes had become sexually inactive testicular tissues from normal apes were transplanted into five of the eunuchs. Into the sixth, a cryptorchid human testis was transplanted. Four of the five males gradually regained potency and again copulated when placed with females. One of the five, into which a testis of a different species of ape was transplanted, failed to regain the sexual libido lost through castration. The sixth into which the cryptorchid human testis was grafted regained potency within six weeks. Although some of the records were incomplete when the report was made, it may be said that libido and reflexes apparently normal in character persisted in some cases for a year or more after the transplantations; in other cases, gradual loss of sexual ardor and impotency were apparent within the first year. Similar results have been reported by various experimenters working with female rats and guinea pigs (Martin, 23; Marshall, 27; Lipschütz, 25).

Restoration of sexual receptivity in rats has also been accomplished by repeatedly injecting into castrated females the follicular fluid from other animals in oestrus (woman, pig) (Allen and Doisy, 2). After this treatment the female becomes sexually receptive as in the normal period of rut. Without gonadal treatment ovariectomized rats are never thereafter found sexually receptive so far as the present experience of laboratory workers goes. This substance taken from the ovarian follicle is, therefore, an active agent capable of bringing about typical expression of the female's "sexual instincts". Similar findings are to be expected in senile animals that have passed the menopause. An investigation of this point is now in progress in the laboratory of J. R. Slonaker, Stanford University. Restoration of copulatory ability and fertility in hypophysectomized rats of both sexes has been accomplished by the injection of fresh hypophyseal tissue or by the method of experimental parabiosis (Smith, 45, 46).

Homosexual Behavior

Hermaphroditic behavior experimentally produced in animals was first brought to the attention of scientists by the publication of Steinach's paper on experimental hermaphroditism in 1912. His work has been repeated many times during the past ten years (Lipschütz, 25). So far, however, little work has been done by way of carefully observing laboratory animals for the purpose of determining the frequency of its occurrence in normal animals. Obviously its occurrence must be carefully checked in all laboratory animals if one would accurately discriminate between what is produced by the experimental methods and what is natural to the animal. Avery (3) noted that homosexual behavior occurred in no less than 10 to 15 per cent of female guinea pigs during the receptive phase of oestrus. It occurs with great frequency among males when these are kept in isolation from females. A male when mounted, however, usually does not take the copulatory posture of the female. This type of behavior was described by Stone (49) in male rats. Two animals that took the characteristic posture of the female when mounted by other males were reported in this paper. When put with receptive females they demonstrated a strong sexual libido and begot young.

That the problem of psycho-sexual hermaphroditism has practical as well as theoretical implications is brought out by the clinicians Hirschfeld (23) and Mott (33), working in the field of psychopathology. They have raised the question as to whether some of the

well known sexual perversions of man are explainable in terms of failure or bisexual functioning of the gonads.

Sex Differentiation and the Gonadal Hormone

Recently much evidence has been brought forward to show that the problem of sex differentiation in the higher animals is not completed until a hormone from the gonad has exerted its influence on the development of the individual (Lipschütz, 26). According to the exponents of this view, the embryo is sexually neutral, so to speak. Whether it becomes a male somatically speaking and exhibits masculine behavior, or whether it becomes a female and develops characteristics typically feminine depends primarily upon the incretory functions of the primitive testis or ovary. For obvious reasons this important theory cannot be further developed in this paper but before passing from the subject, we wish to call attention to a striking case that has some bearing on this theory. Crew (15) reported a case of complete sex reversal in a barnyard fowl. According to his report a hen with typical hen feathering laid and brooded eggs, and also took care of young chicks. Afterward the ovary became diseased and the essential ovarian tissue disappeared. Still later she developed testicular tissue together with tubal accessories appropriate to delivering spermatozoa into the cloaca. With this new sexual apparatus appeared the characteristic behavior of the cock; he crowed, treaded females, and fertilized eggs. In short, there was complete reversal of the psycho-somatic characteristics of the normal hen.

Voluntary Activity During Oestrus

In his studies of the rat's voluntary activity from infancy to senility Slonaker (1912) noted periodic fluctuations in the daily activity of mature females. At the time he was unable to account for these fluctuations or correlate them with any known factor of environment. After Long and Evans (26) demonstrated a normal four- or five-day interval between oestral periods in rats the probable factor underlying these periodic fluctuations of activity was suggested. Wang (61) and Slonaker (42) independently demonstrated a temporal correlation between increased daily activity and the receptive phase of oestrus by confining the females in revolving cages. Prior to puberty there are no well defined peaks in the activity curves, but as puberty approaches small peaks appear irregularly, suggesting mild influence of the ovarian hormone before the first oestrus. A short time after the age of puberty, these peaks appear at

four- or five-day intervals when rats are maintained on a well balanced diet. As the menopause approaches, these peaks again become irregular and eventually disappear altogether. The menopause usually comes at the end of their second or the beginning of their third year (Slonaker, 44).

Since the first publications of Wang and Slonaker appeared several other studies of voluntary activity associated with various aspects of the reproductive functions have been reported. According to Slonaker (43) the albino rat's running in the revolving cages is chiefly nocturnal and in this respect is little influenced by the presence or absence of oestrus. The main activity each day occurs between 8 P.M. and 10 A.M. with the peak falling between midnight and 4 A.M. The oestral peaks result from augmented activity within a short period of time rather than the spread of activity over a longer period of time. Activity of the preoestral and the oestral periods are similar; likewise activity of the posteostrus and dioestrus resemble each other. During the receptive phase of oestrus activity is usually continuous. At other times it may be interrupted by rest periods of one or more hours duration. In all stages of the oestral-dioestral cycle the major portion of daily activity occurs during the early phase of nocturnal activity with a somewhat gradual shading off toward the inactive period of the day. Slonaker further observed that the food consumption of females during oestrus varies inversely with these rhythms of activity. During the latter days of gestation in pregnant females the amount of activity decreases. The decrease is especially great on the last day. After successful coitus occurring when the female is extremely active there is a characteristic drop in activity. This drop can usually be relied upon as an indication of mating. Mothers carrying large litters are more active than mothers carrying small litters. Likewise mothers nursing large litters are more active than the reverse. During the period of gestation typical periods of activity correlated with oestrus are absent and, as a rule, do not reappear during the first twenty days of lactation. A few cases in which one or more cycles of activity occurred during gestation and lactation were found, however.

According to Wang, Richter and Guttmacher (64), there is a distinct sex difference in voluntary activity in males and females. Normally a male does not present the periodic fluctuation coming at four- or five-day intervals so characteristic of the female. Ovarian grafts, however, may feminize the male with respect to activity. Males were castrated and ovaries transplanted into the muscles of

the abdominal wall. The grafts that established a blood supply were not absorbed; otherwise they were absorbed. In all cases where ovarian grafts took root an increase in the male's activity occurred. The amount was variable, but greatest in those cases wherein the ovarian tissue recovered at autopsy most closely resembled normal ovarian tissue. In some instances characteristic spurts of activity similar to those of the female in oestrus were found. Their recurrences, however, were not as regular as those of the female.

When the ovaries of a female are removed (Wang, 61) the amount of voluntary cage activity drops about 90 or 95 per cent. Recently Bugbee and Simond (7) injected follicular fluids from ovaries to determine whether cage activity typical of oestrus could be revived in ovariectomized animals. Their experiments led to the following conclusion: "Injections of extracts of ovarian follicular hormone, in proper dosage and at proper intervals, into ovariectomized rats cause artificial heat periods in which the voluntary activity is increased the same as in normal heat periods in normal females. Repeated injections of extract of ovarian follicular hormone increase the total voluntary activity of ovariectomized rats during the period of injection, but when injections are discontinued the voluntary activity decreases to its former low level."

In addition to the female's variations in daily activity which are temporally and causally correlated with the factors underlying oestral rhythm, Slonaker (44) finds plateaus of daily activity that extend over long periods of time—200 days or more. Upon these long-time fluctuations daily fluctuations are superimposed. The long-time fluctuations occur in both sexes but there is a lack of synchronism of these plateaus in different animals simultaneously observed. Hence, it is unlikely that they are caused by changes in the room temperature, moisture content of the air, or barometric pressure or other factors fluctuating with seasonal changes. In observations extending over a period of 905 days it was found that these plateaus of activity are dependent upon internal factors inherent in the rat and are of normal occurrence.

Maternal Behavior

Avery (4) reports case histories of parturition in guinea pigs as observed both in primipara and multipara. "The primipara demonstrates in parturition a variety of complex responses concerned with the delivery of young, rending of foetal membranes, consumption of membranes, and cleansing of young. The efficient manner with

which these functions are performed without previous experience or tuition, illustrates the fundamentally innate nature of initial maternal responses." His observations show that the repertoire of maternal activities devoted to the care and protection of her young is much less elaborate than that of other laboratory animals, such as the rat, mouse, rabbit, or cat. She does not make a nest, bring her young together by carrying them in her mouth, or hide them in out of the way places. The well developed condition of the young at birth, however, makes this unnecessary.

A preliminary note on maternal behavior of one of a pair of sister rats living in a state of parabiosis was reported by Stone (53). Previous experimental observations (unpublished) led him to the belief that maternal behavior in the rat was conditioned in some way by physiological factors attendant upon pregnancy, parturition, and nursing of young. In the absence of some or all of these physiological factors, stimuli of the external environment do not arouse maternal behavior toward young rats. This experiment was designed, therefore, to ascertain whether a chemical substance (hormone) elaborated in an animal that carried to term and delivered young might pass into the circulatory system of the other and, in her, arouse maternal behavior toward the young animals delivered by the real mother. That the juncture of the two animals permitted diffusion of substances from one to the other was conclusively demonstrated by injecting Trypan blue (an aqueous suspension) into one of the two and noting that it quickly passed over to the other animal.

These animals lived in a state of parabiosis slightly over one and a half years. During that time one of the females bore three litters and exhibited the usual maternal behavior toward her young; the other animal, however, did not show any disposition to handle the young, bring nest materials together, etc. So far as causing maternal behavior in a virgin female by giving her a common circulation with another rat that went through pregnancy, parturition, and lactation this experiment was negative. No forecast is given as to the ultimate success of this type of experimental work.

Rogers' study (41) of the rearing of young by decerebrate pigeons is reserved for the section dealing with native responses in animals deprived of portions of the central nervous system.

CONGENITAL BEHAVIOR FOLLOWING CEREBRAL LESIONS

The study of Martin and Rich (29), although appearing prior to the time my report is intended to cover, is briefly reported here

because it does not seem to have gotten into the psychological literature. Working with young chicks these authors removed parts of the cerebral hemispheres to varying depths: (1) the shallowest lesions were confined primarily to the pallium; (2) others included the pallium and parts of the corpus striatum; and (3) others still were sufficiently deep to involve the thalamus. Chicks decerebrated immediately after hatching according to the second procedure develop locomotor and self-cleaning activities as early and substantially as effective as normals of similar age. Although they begin to peck about as early as normals they fail to progress beyond the act of pecking to successful seizing of food. Scratching in litter develops very slowly and spontaneous drinking fails to appear.

When chicks were decerebrated (method 2) between the third and eighth days, *i.e.*, after the normal reactions were well established, their abilities reverted to the condition of chicks decerebrated immediately after hatching. However, these chicks resumed the activity of scratching in litter more promptly than it is developed in chicks that had not scratched at the time of decerebration.

When the injury was confined to the pallium only minor differences between the operates and the normals were observed. Those chicks in which the operation included injuries to the thalamus (method 3) appeared weaker and had less stable equilibrium than those with shallow lesions. The walking was unsteady and the acts of preening were accomplished with difficulty. From these experiments, "the conclusion is drawn that the cerebrum has no necessary concern in the development and mediation of locomotor and self-cleaning activities in chicks. The successful accomplishment of feeding depends on the coöperation of the cerebrum, and the simpler phases of the act, pecking and scratching, are normally developed through the coöperation of the cerebrum, although if this is removed early enough in the life of the chick both may develop independently of it." Martin and Rich did not determine the extent of lesions by histological studies of the chick's brains.

Rogers (41) has made an extensive study of the instinctive acts of partially decerebrate pigeons. This bird has a poorly developed cerebral cortex but a relatively large and complex corpus striatum. After the observations were completed, he determined the extent of brain lesions histologically. I quote from his summary: "The following cycles or reactions were observed in adult birds before and after various cerebral lesions were made; feeding, drinking, mating, nesting, incubation, and rearing the young birds. *A few observations*

of the development of the behavior cycles were made in birds in which cerebral lesions were made in the first month of life." (Italics mine.)

"Loss of all but traces of the cerebral cortex but leaving the major part of the hyperstriatum intact is followed by no characteristic behavior deficiencies; such birds fed and protected themselves, mated and reared young in the normal fashion."

"After loss of cortex and hyperstriatum there follows a long period of helplessness, but the bird may regain the ability to feed itself, but does not go through the mating or nesting cycles of behavior."

In three papers Stone (55, 56, 57) described the effects of cerebral destruction on the sexual behavior of male rabbits. The first paper dealt with destruction of the olfactory bulbs; the second, with the frontal and parietal lobes; and the third, with the frontal, parietal, and occipital lobes. From the work done so far, he concludes that, "The male rabbit does not require the olfactory bulbs, or the cortex of the dorsal and dorso-lateral convexity of the cerebral hemispheres for the manifestation of strong sexual libido and the execution of the copulatory act. Strong sexual libido and likewise fertility may persist as long as six months (duration of experiment) and probably much longer, after the rabbit has received very extensive lesions of the cortex."

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LITERATURE ON THE HEREDITY OF BEHAVIOR TRAITS IN ANIMALS¹

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The inheritance of behavior traits in animals is a subject of great interest and importance to students of animal psychology as well as to geneticists. Nevertheless, very little fundamental work has been done in this field by investigators thoroughly conversant with the literature on heredity or familiar with the experimental methods of the geneticists.

The publications cover a wide range of problems in heredity without much overlapping or repetition. For ease of discussion they may be classified under three heads: (1) articles dealing with heredity of learning ability; (2) those covering the heredity of temperamental or "character" traits; and (3) those concerned with the transmission of degeneracy from parent to offspring. Investigators have considered the heredity of traits from the standpoint of resemblances of siblings, resemblances between parent and offspring, the effects of inbreeding on learning ability and temperamental traits, and the individual and group differences between offspring of alcoholized and normal parents. As the following citations show, most of the investigations do not touch directly upon the fundamental mechanisms of inheritance.

The Inheritance of Learning Ability. Basset (4) studied the maze learning ability of a strain of inbred rats with less than normal brain weight. The animals were taken from the 6th, 7th, and 8th generations of Miss Helen King's experiment on inbreeding conducted at the Wistar Institute of Anatomy, and their brains were subnormal as figured either in relation to body length or body weight. He used the Watson circular maze and an inclined plane problem box as training devices.

The animals with subnormal brain weight were inferior to the normal controls when compared on the basis of (1) the average number of trials required to satisfy his criterion of complete learning;

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(2) the number of each group failing to satisfy his criterion of learning; and (3) the minimum number of trials required by any animal to learn the problem. In addition, it was found that the animals of the 7th inbred generation learned less rapidly, using the foregoing criteria, than those of the 6th. Likewise those of the 8th learned more slowly than those of the 7th.

The studies of Bagg (2, 3) on individual differences and family resemblances in mice give data on the relation of siblings in respect to maze learning ability. He used a simple maze-like box in which the animal was required to make three choices of exit doors per trial. His animals were a heterogeneous assortment of various races, consisting principally of albino, gray, agouti, chocolate, and yellow mice. Among his earlier conclusions is the statement that the mean variation of learning time was much smaller for the litters than for the group as a whole; in fact it was enough smaller to be considered equivalent to a coefficient of correlation of 0.5. No one up to the present time has published data indicating a similar degree of resemblances for learning ability of animal siblings and the author himself, after correcting for sizes of the litters, found no family resemblance. Bagg gives a large number of pedigree charts illustrating the learning ability of different animals derived from crosses of slow, fast, and intermediate learners, but he makes little use of them by way of formulating conclusions concerning the transmission of learning ability through successive generations. However, different strains of mice appeared to show differences in learning ability.

Tolman (11) has studied the relationship in rats between parents and offspring in respect to maze learning ability. The reliability of his maze, as determined by correlating odd with even trials, was 0.37. Waiving entirely the question as to whether a correlation of odd with even trials gives a suitable measure of reliability, it is obvious that the foregoing coefficient is very low. Hence the maze was probably inadequate for an exact and detailed study of family resemblances wherein it is assumed that the learning scores of individuals are relatively true measures of learning ability. Having trained a group of animals, he then selected small groups of the brightest and dullest, and interbred them within their respective groups. These offspring in turn were trained on the maze, and from the resulting data, comparisons between parent and offspring were made. The offspring of the bright parents had a lower mean number of errors than their parents, and likewise were more closely grouped around their mean. For the dull group the mean number of errors was not quite as high

as that of the parents, but their variability was somewhat greater. A third generation was secured from selective breeding of the bright and dull animals of the second generation. Results for this generation were similar in general trend to those of the first and second generations. Hence the experiment rather clearly demonstrates the possibility of producing a relatively bright and a relatively dull group of animals by selective breeding of trained animals.

The Inheritance of Temperament. The study of Yerkes (14) appears to have led the way for, and strongly influenced the succeeding studies of temperamental traits in rats and mice. In his pioneer study he arbitrarily defined a graded series of standards of savageness, wildness, and timidity, and judged the animals from these standards. His animals were kept in laboratory cages and cared for in the usual way except at the time of testing when each was removed from its cage with a gloved hand or placental forceps for observation. The tame rats were crossed with the wild ones, and their offspring tested. He observed that the offspring of the tame by the wild rats were less wild, timid, and savage than their wild parents, but much more so than their tame parents. In short they occupy a relatively intermediate position between parents. The second generation of hybrids resulting from intercrosses of first generation hybrids were less wild, timid and savage than their parents. In the hybrids of the first generation the offspring were slightly nearer their wild than their tame parents, while those of the second generation were nearer their tame parents.

Ada Yerkes (13) observed the differences in learning ability and temperament between inbred and normal rats. She studied their learning ability on the Watson circular maze and the Yerkes discrimination box. The data on temperament were obtained by observing the animals in their cages as well as their behavior in experimental situations. Only a small difference in learning ability was found; in fact it was less than the sex difference. The male controls learned the maze in fewer trials than the inbred males, but traveled greater distances, took longer time for the trials, and made more errors. The female controls were inferior to the female inbreds in number of trials required to learn the maze, but superior in distance traveled and time per trial. When the data for both sexes were combined, the mean number of trials of the controls was lower than that of the inbreds, although the difference was small and *in all probability not reliable*. On the discrimination box the

inbred animals required a larger number of trials to form the habit, but were able to discriminate smaller differences than the controls.

Observation seemed to show greater differences in temperamental traits between the two groups than was shown for learning ability. The inbred animals were timid, sensitive, inactive, lacked initiative, and showed instability of behavior, while the controls were diametrically opposed to them in these traits. Of the traits studied, the differences for timidity and instability of behavior were most marked.

Utsurikawa (12) studied temperamental differences between inbred and outbred rats in two ways, namely: observation under natural cage conditions and under experimental test situations. Both were observed under identical conditions. For the experimental observations, the animals were put individually into a small black box with suitable kymograph attachments to insure that the animal's movements were recorded on a revolving drum. These records revealed differences in type of behavior both when the animals were left undisturbed and when subjected to special stimuli. The outbred animals came forward more frequently in their cages, were more active, less savage, and less attentive and responsive to auditory stimuli. They were more restless under continuous stimulation than the inbred, but less restless under intermittent and momentary stimulation. The author commented on the smallness of differences between the two groups, and showed that for each series of observations the variability within groups was greater than intergroup differences.

Coburn's study of wildness and savageness in mice (5) closely follows the method of Yerkes as to technique of rating traits and program of breeding animals, but is carried out in greater detail. Hybrids of the first generation received lower mean and modal grades for savageness and wildness than their wild parents, but higher grades than their tame parents. Hybrids of the second generation were slightly less wild and savage than those of the first. Coburn combines the two traits into four classes, as follows: wild-savage, wild-nonsavage, tame-nonsavage, tame-savage. Each animal is tested and put into one of these classes. The largest number of cases falls under the first classification, the next largest under the second classification, and so on. From a consideration of the general data of the study the author infers that the two behavior complexes are the result of the blending type of multiple factors.

Phillips (10) made observations on the tamability of crosses between Mallard and Black ducks, which are closely related species.

The Mallards are tame in captivity, although controls hatched in secluded spots are wild. The Black ducks are very wild, and even in captivity do not become tame. He found that crossing the two species to produce hybrids three-quarters Black and one-quarter Mallard gives wild and untamable ducks, whereas three-quarters Mallard and one-quarter Black gives ducks that are wild in infancy, but become tame with age and training.

Inheritance of Degeneracy. Stockard and Papanicolau (9) attempted to analyze the hereditary transmission of degeneracy and deformity in the descendants of alcoholized guinea pigs. This paper does not deal with temperament or learning ability, but deserves mention because the authors attempt to causally relate the deformities of the nervous system induced by alcohol with injuries of the germ plasm. They found that the female offspring of alcoholized fathers had a higher rate of mortality and per cent of deformities than the male offspring, but that the reverse was true for the offspring of alcoholized mothers. On the basis of these and other pertinent data the authors assert that the alcohol affects the sex chromosomes differentially. Either because of greater mass or chemical affinity, the X chromosome is more affected than the Y.

Another possible explanation for at least some of Stockard and Papanicolau's data was suggested by Cole and Ibsen (6). They found a definite neurosis in guinea pigs consisting of clonic spasms rendering the affected animal helpless. The disease runs a short and invariably fatal course. By breeding experiments the authors found that this disease is inherited as a Mendelian recessive. Matings of heterozygotes produce three apparently normal animals to one affected one. Matings of known normal animals with known heterozygotes produce equal numbers of normal and heterozygotes. This study is particularly interesting in consideration of that of Stockard and Papanicolau, for the latter got deformities closely resembling paralysis agitans among their alcoholics. Cole and Ibsen suggest that Stockard and Papanicolau may have uncovered such a trait as their own, in their alcoholic strain and continued it through inbreeding, while attributing it to the effect of alcohol because it came to light in that strain.

Arlitt (1) studied the effects of alcohol on maze learning in rats. A moderately difficult maze was used. She found that animals that had previously learned the maze, ran less rapidly but with approximately normal accuracy after alcoholization. Large doses increase the retardation of running. Four out of seven groups of nonalcoholized offspring of alcoholized parents were markedly inferior to the

offspring of normals with respect to number of trials required to learn the maze, total time, and total errors. Three of the seven groups were equal to or superior to the offspring of normals on one or more of the criteria. The parents of these three groups had received only light doses of alcohol and were alcoholized over a shorter period of time than the parents of the other four groups. The most proficient of these three groups was derived of parents, of which fathers only had been alcoholized.

More defectiveness in learning ability is found in the third generation of alcoholized parents than in the second. The offspring of a cross between normal and alcoholized parents are superior to the alcoholic parents, and in a small per cent of the cases, superior to the normal parent. (The author does not demonstrate that her differences are reliable.) The majority of alcoholized parents appearing in the fourth generation were derived of lightly alcoholized stock. These individuals showed only a slight defectiveness of learning ability.

Autopsy of the alcoholized parents showed degenerative signs in the reproductive processes, particularly in spermatogenesis.

McDowell and Vicari (9) studied the maze learning ability of nonalcoholized descendants of alcoholized grandparents. The grandparents were subjected daily for a long period of time to alcohol vapors in a fume tank. These animals were inbred to produce the experimental line, while their unalcoholized brothers and sisters were inbred to produce the control line. The grandchildren of both lines were tested on the Watson circular maze. He found that the animals of alcoholic lines were inferior to their controls. They required more trials, made more errors, and made fewer perfect runs. In one phase only did there seem to be no clear difference between the groups; that was in the speed of running as measured by the number of centimeters covered per second. The authors failed to find morphological abnormalities in all but one line; in this line abnormalities appeared in both the experimental and control groups. Obviously they were not an effect of the alcohol.

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SPECIAL REVIEWS

ROBERT M. YERKES. *Almost Human*. N. Y.: Century, 1926. Pp. xxi+273.

"Almost Human" is a valuable contribution from the viewpoint of scientist and layman. Yerkes has made available for the student of animal behavior the results gained by Madam Abreu from years of companionship and observation of her collection of primates which number seventy-five and represent most families of Anthropeidea. The chapter "Who Is a Primate?" fills a gap in the reader's knowledge of the higher apes and monkeys giving a brief account of the characteristics, habitat and habits of the various species. The next portion of the book is given over to a discussion of the intelligence, emotional life, and speech of the anthropoid. In this discussion Yerkes refers frequently to the work of Koehler, Kohts, Garner, and Yerkes as well as the observations of Madam Abreu. For the general reader or professional psychologist who wishes a passing acquaintance with the field this is a good review. Madam Abreu's observations are reported by Yerkes in an anthropomorphic fashion, perhaps for the sake of accuracy of report, or perhaps out of a sense of obligation. From the point of view of the layman this is unfortunate since there is already present a tendency to attribute our own feelings, emotions and motives to the behavior of animals. In this connection Yerkes from time to time points out the need for further observation and control before accepting the humanized interpretation of the animal's behavior. The following chapters deal with the care of captive primates and should be of value to any worker or layman who contemplates keeping a primate colony. The closing chapter forcibly brings out the excellent field for experimental studies, psychological, medical, and educational, presented by the infrahuman primates.

Written in a style which the layman can readily understand, "Almost Human" is of social significance at the present time. The lay reader is constantly bombasted with evidence for and against evolution, most of which is morphological and structural. The behavior descriptions of the infrahuman primates corroborate the facts of evolution in an almost uncanny, and certainly to the fundamentalist, uncomfortable manner. Indeed, in the behavior of some animals, the fundamentalist has his compliment returned; to wit, "In

physique, many of the more lowly primates are caricatures of man. The resemblances are too striking to be ignored and they seem to be recognized even by certain of the monkeys and apes, which on occasion also resent them!"

CARLYLE JACOBSON

University of Minnesota

FRIEDRICH HEMPLEMANN. *Tierpsychologie vom Standpunkte des Biologen*. Leipzig, 1926. Pp. viii+676.

Dr. Hemplemann is a professor of biology and comparative anatomy at the University of Leipzig. Why he chose to write on psychology rather than biology or anatomy, he does not say. Perhaps because it is a more fascinating subject? The author's reason as set forth in the preface seems to be that the time is ripe for such a book, and the wealth of material which has accumulated during the last ten years, and particularly during the last year needs evaluation and classification. Except in reference to important issues the book is limited to the literature of the last decade and in the field of sensory processes only the very important papers are reviewed.

The author feels and we hope that other biologists agree that there is now no question that there is such a science as animal psychology. No longer as in the past shall we have to argue that point, but assuming it, can proceed by experimentation to establish it on ever firmer foundations.

The author proposes to set forth all sides of animal psychology where it is feasible to do so. It appears to the reader that when it came to theoretical points of view, it was seldom feasible. The author is an out and out "Gestalter." His third reason for writing the book is to do justice to this fertile theory, the Köhler-Wertheimersche Gestalten-Prinzip, which has at last found its way into comparative psychology. Several pages are devoted to the development of this theory and to a statement of it which the reviewer thinks is probably as clear a statement as we have anywhere. The theory smacks too much of structuralism, perhaps, to ever prove very useful to American comparative psychologists. However, the reviewer scarcely dares go as far as Dr. Hemplemann, who lumps us all together and naming no names, damns behaviorism with faint praise in the sole statement which follows, "Eine besonders rege Tätigkeit entfalten amerikanische Forscher auf dem Gebiete der tierischen Gebarenslehre, so dass wir diesen 'behaviorists' eine schier unübersehbare Menge von Einzelergebnissen verdanken, die zumeist das Lern-und Assoziationsvermögen der Tiere betreffen." In this statement is the cue to

the difference in point of view of this author and most American psychologists who are interested in the animal field. We have aimed to study learning and association and instincts, if the term may still be used, instead of animals but Hemplemann is everywhere interested in the animal first. However, all this occurs on page 627. Perhaps it would be well to go back a bit.

The book starts with an introduction which as is customary includes Romanes, Darwin, Lamarck, etc., and a number of German authors not so often included. It is in this chapter, too, that the author makes his excuses and discloses his beliefs. He says, *e.g.*, page 7, "Wir werden in den folgenden Ausführungen dieses Buches dem Sprachgebrauch folgen und von Empfinden, Wahrnehmen, Sehen, Hören usw. der Tiere sprechen, ohne damit sagen zu wollen, dass diesen Vorgängen jedesmal etwas Psychisches einhergehen müsse." If he had left out *jedesmal*, we might have made a behaviorist of him yet but *jedesmal* is a loophole which no behaviorist would permit himself. He further intends to make use of the conception "serving a purpose." This is not to be understood, however, in the teleological sense but "im Sinne von erhaltungsmässig, nützlich, lebensfördernd, ökologisch usw., also immer vom Standpunkte des Beobachters, nie vom subjektiven des betreffenden Tieres aus."

The arrangement of the first part of the book is interesting. It is the customary arrangement of the zoölogy textbook and bespeaks an evolutionary point of view. Beginning with the protozoa it proceeds by the biological steps to the mammals. This takes 454 pages. According to the author it lies in the way of the animal organisms themselves that in the protozoa and lower metazoa, physiology must come more into the foreground than in the higher developed animals. In this part of the book we have principally a review of the literature, especially of the German literature which should be of immense assistance to the American student. Hemplemann also includes considerable American literature, particularly of the biologists, Jennings, Mast, W. H. Cole, and W. M. Wheeler. Yerkes also comes in for his full share, there being no fewer than sixteen separate references to his work. Carr, Hunter, Watson et al., are quoted.

Under each animal genus we have a description of the species chosen for illustrative purposes, their natural behavior, and then a review of the particular articles which have reference to them. The beginnings and development of the nervous system and brain are

followed through in detail. Everywhere there is correlation of structure with function and the author closes this part of the book with a quotation from Köhler which follows: "Soweit stimmen die Beobachtungen gut zu den Erfordernissen entwicklungsgeschichtlicher Theorien; insbesondere bestätigt sich Korrelation von Intelligenz und Grosshirnentwicklung."

The second part of the book seems a strange mixture of fact and fiction, of scientific acumen and naïvité. Hemplemann speculates on the content of animal mind and even of consciousness, even while he denies himself the privilege. In the introduction he outlines this part of the book as follows. After speaking of the earlier part of the book he says: "Da wir auf diesen Wege zugleich auf eine Anzahl von Erscheinungen stossen werden, für welche das Leben des Menschen durchaus Analoges aufweist und denen beim Menschen Bewusstseinsvorgänge einhergehen (Übung, Lernen, Assoziationsfähigkeit, Gefühlssäuszerungen), so erhalten wir zwar nicht eine objektive Kenntnis von der Psyche der Tiere, doch können wir uns auf diese Weise wenigstens ein Bild davon machen, welche Vorgänge sich im Bewusstsein der Tiere abspielen möchten, wenn wir sinngemässe die uns von uns selbst bekannten entsprechenden Bewusstseinskorrelate einsetzen, ein Bild, das die geistigen Fähigkeiten der Tiere zum mindesten nicht übertreibt, wahrscheinlich sogar noch unter der Höchstgrenze ihrer psychischen Leistungen bleibt." The author, fortunately does a great deal more than this in the latter part of the book.

In the first place we have really excellent discussions of vision and orientation in animals. Here as everywhere we find splendid illustrations. In all there are 134 in the book. There follows short discussions of the time sense, counting, infancy and play in animals. These subjects rather follow the conventional treatment but include what experimentation there is on these topics.

Now we come to the most intriguing part of the book. If the author makes somewhat free use of the words, mind, consciousness, instincts, feelings, etc., it is probable that, contrary to the current American opinion, nobody will fail to understand to what he refers. This section is called "Das Gefühlsleben der Tiere" and the author first discusses feelings and affections. He reviews the literature, beginning with Wundt's chart on the changes in pulse and breathing during emotional states. Hemplemann seeks physiological criteria of feelings by means of which we can determine their presence. He discusses the characteristic attitudes of various feelings, quoting the

work of Dexler, Buttel-Reepen, Köhler, v. Uexküll et al. These are accompanied by excellent pictures of animals depicting various feelings. Notable among these is a double page containing 8 photographs of an ape in different attitudes presumably portraying different emotions. Using the same signs we are accustomed to use in every day life, it is quite easy to select the proper feeling and I have no doubt we would find at least as much agreement as with human photographs. The difficulty is in determining how the ape really felt during the pictures.

In this division he also discusses speech, ethics and aesthetics in animals. We would like to review each at length but we must be content with a few references. Under the topic of speech we find a significant statement. "Der grosse Unterschied zwischen unserer menschlichen und der tierischen Sprache besteht darin, dass den Tieren die Darstellungsfunktion des menschlichen Sprechens fehlt." Just below this the even more significant statement, "Die Tieren haben keine Wortsprache, da bei ihnen das begriffliche Denken fehlt." Under Ethics the author discusses sex relationships, perversions, etc., and under aesthetics, song, dance and the like. He says of the latter that only the higher animals truly display them but a lack of these feelings is also important for, "Vermögen wir damit doch mit Sicherheit zu sagen, welche psychischen Vorgänge und Bewusstseinsinhalte den Tieren nicht zukommen," which is surely a unique way of determining mental content—by what is not there.

The final section of the book, except for the history and methods of animal psychology, is called "Analogien zu besonderen psychischen Zuständen des Menschen," Hemplemann never seems to be as worried about the term analogy as American writers are apt to be, which in a way is something of a relief. Under this heading we have sleep, dreams, hypnosis and mental disorders. It is impossible to do justice to these topics here. All are treated in an interesting manner and if the reviewer has some of the American qualms in regard to analogies in general, that is probably due to too much behaviorism. The review of the work on hypnosis in animals is particularly noteworthy and here again we have excellent photographs of all sorts and degrees of animals in the hypnotized state.

Following this is "Das Gehirn als Organ der Psyche," including the development of the brain, etc., "Die Psyche," concerned with criteria of mind, and a short history and discussion of methods. This is principally a summation of what has already been indicated throughout the text. The book ends in true literary style, "In der

Tierpsychologie gilt daher mehr denn sonst der Satz: Das Masz aller Dinge ist—der Mensch!"

There is no doubt in the mind of the reviewer as to the value of this book. It is both excellent and entertaining. Hemplemann is a master of arrangement; the whole treatise, considering the truly amazing amount of material included, proceeds clearly and logically. No point of view is allowed to dominate the book to the extent that any material is denied entrance. From the standpoint of mechanical form, the book is really a work of art. The extreme care which the whole work shows is further displayed by the classified bibliography and the very complete indices. The reviewer feels that the book is worthy of a great deal more study than was possible before writing this review and apologizes to Dr. Hemplemann for her shortcomings on those grounds.

OAKLAND M. HERON

University of Minnesota

ROBERT SOMMER. *Tierpsychologie*. Leipzig: Quelle & Meyer, 1925. Pp. i+245.

As the author states, this book is not an exhaustive treatment of animal psychology but is rather an attempt at a general orientation of the subject. His expressed hope is that the book may help create an interest in the psychology of animals because of its bearing not only on human psychology but also on human psychiatry.

Beginning with a short historical sketch, he proceeds to a consideration of the similarities and differences existing between the human and the lower animals. Among these he discusses the erect posture as well as the differences of the anatomy of the extremities and endeavors to point out some of the results of these differences in the mental life of human beings and animals. Other similarities and differences are brought out in the discussion of the expressive movements and the organs and muscles upon which they depend. Under this heading he also includes and discusses the psychophysiology of speech which leads him into a discussion of the anatomy of the brain. In discussing these similarities and differences, Sommer impresses upon the reader the necessity of having a rather accurate knowledge of the anatomy and physiology of the animals under discussion, for the mental life of the animal is determined by the anatomical structure which is present. Throughout the discussion he stresses the fact that in mental life as in physical life the differences found are due to differences in evolutionary development.

He next considers the psychological categories such as: sensation,

memory attention, ideas, instinct, intelligence, association, abstraction, apperception, and habit. In each case he attempts to define to what extent they are found below the human level and points out as mentioned above that this extent is determined by the evolutionary development of the animal. In the following pages he takes up the various species of animals and discusses their mental life.

Probably the most important part of the book is the latter part in which Sommer discusses the comparative pathology of internal hydrocephalus and the comparative psychopathology of man and animal. Under the latter heading, he discusses the following: convulsions (Krampf), a form of dementia praecox (Katatonische Symptome), emotions (Affekte), distraction (Verwirrtheit), deliriums and fixed ideas (Sinnestäuschungen und Wahnideen), and psychogenic symptoms (Psychogene Symptome). Here, as mentioned above, he endeavors to point out the importance of animal psychology and especially animal psychiatry in its relation to human psychiatry.

In addition the book contains quite a detailed description of a study of the Elberfeld horses made by Sommer and others in 1914. Most of the experimental work mentioned is drawn from German sources. There are given also many observations made outside of the laboratory. Foremost among these are the observations made on a horse purchased by Sommer after the World War and which he observed closely over a period of several years. The last ten pages of the book are devoted to a bibliography on animal psychology again selected almost entirely from German sources.

W. G. McALLISTER.

University of Minnesota

MARGARET FLOY WASHBURN. *The Animal Mind*. A Textbook of Comparative Psychology. 3rd Edition. New York: Macmillan, 1926. Pp. xiii+431.

Again, after another nine-year period, we have a revision of Miss Washburn's book: *The Animal Mind*. This revision, like the first, is chiefly a matter of incorporating the new material which has accumulated. So far as the present reviewer can see, Miss Washburn's attitude toward her subject-matter has not changed appreciably.

Of course, if one is inclined to be argumentative, one might find grounds for the assertion that a nine-year period of struggling against the tidal wave of behaviorism has had its effects. For example, compare these two sentences! The first is from the second edition (page 24): "Our object in this book will *always* be the interpretation of the inner aspect of the behavior of animals; we shall be interested

in what animals do *only* as it throws light upon what they feel." The second is from the third edition (page 21): "*One main object in this book will be the interpretation of the inner aspect of the behavior of animals; we shall be interested in what animals do largely as it throws light upon what they feel.*"

One might assert on the basis of this comparison that Miss Washburn is coming to believe that, "To the *true* psychologist" there is another challenge which is, at least, almost as enticing "as that presented by the problem of how it feels to be another person or another animal." * This other challenge is to study the behavior as a sufficient end in and of itself. This is a problem which we need never give up in despair as Miss Washburn admits to be sometimes the case with the problem of feeling by proxy.

On the other hand, to one who is not interested in arguing a question on rather slight evidence, the change of a few words in these two sentences may only signify that the author has corrected in the present edition a slip of the pen in the last. Although we are not told explicitly of any other main object of the book, yet one suspects that the author seeks to include all the important work on animal behavior, regardless of whether or not it throws any light upon the nature of the animal mind, and regardless of whether or not, it was done by one who had that as his primary interest. It is this objective which the reviewer feels that the author has most fully attained.

In fact as one reads the book, he is reminded of Dorsey's question (in *Why We Behave Like Human Beings*): "Washburn's *Animal Mind*, second edition, lists 841 titles consulted in the preparation of her book. That was eight years ago. The next edition will probably list a thousand titles; shall we know more then about the 'mind' of animals?" We find that his prediction of the number of titles consulted falls short by 135. And, with the third edition in our hands, we are ashamed to answer the question, for it seems that the work of the past nine years has added painfully little to the sum total of our knowledge of the minds of animals. Of course, there is added, as the final chapter of the third edition, a discussion called "The Animal Mind." But, in this chapter we find nothing that could not have been written nine years ago, and very little that could not have been written had no one even thought of doing an experiment on animal behavior.

Out of the mass of other material which has been added, we will select only two examples for a brief comment. The first is the

* All italics in the above quotations are mine.

account of the "configurationist" school. The author spends very little time in discussing this school in general. Her chief discussion is with reference to the solution of problems by animals by what Köhler calls insight. She believes that insight "requires in many cases the memory idea, and in many others at least that incipient revival of movements which we have regarded as the basis of a memory idea." She believes further that, "In most cases of insight," . . . "the solution occurs not by introducing an object absent at the outset, but by the emergence into the 'focus of attention' of an object already present but disregarded. Our theory would suggest that this emergence happens because the sight of the relevant objects sets off slight recurrences of the movements by which in past experience they have been reacted to; that is, slight anticipatory movements such as would, if the objects were present, revive memory ideas of them." Miss Washburn seems to believe that there is nothing in the examples of this phenomenon called insight which cannot be described easily and equally well in terms with which we have been familiar for years.

The second item of new material which we wish to mention is the author's theory of the drive which she sets forth in this edition. At the risk of doing the author an injustice, the reviewer feels that the matter is important enough to attempt a summary of her theory. This summary may take the form of listing five points which are made concerning the drive.

1. The drive is a "state of physiological unrest, due either to a lack or a superfluity of certain physiological substances."

2. The first effect of the drive is "to set in readiness the movements that have oftenest followed it, namely, the movements that have put an end to it."

3. If these movements cannot occur, or if they do occur but, for some reason, do not end the state of unrest, then the physiological state "extends its influence further and sets in readiness those movements which have on former occasions occurred just before it came to an end." If these in their turn do not end the drive then it "extends its influence still further backward to the movements which just preceded the last ones, and so on back."

4. What is said in proposition No. 3 does not necessitate the use of the concept of backward association for the reason that "The beginning as well as the end of the maze running process is associated with the drive because *a drive is not a momentary stimulus like a sound or an electric shock, but a persistent physiological state that is present through the whole running.*"

5. "It is the putting of an end to the drive when the center of the maze is reached that produces the elimination of errors."

The reviewer has no objection to make to any of these statements except the last. This seems to be the weak point, for as far as he can see, we are no better off after this statement is made with reference to the question of the elimination of errors than we have been heretofore. On the premises which are laid down, all movements, regardless of whether they are erroneous or not, which occur during the course of the drive may become associated with it. We need a further factor to account for the fact that some are and some are not so associated. How can the "putting of an end to the drive" have such a selective influence? Of course, many selective factors have been proposed, such as frequency, recency, unpleasantness, etc., but all have been shown to be more or less unsatisfactory, else we should have no need of a new theory.

Furthermore, we may look at proposition No. 5 from another standpoint. It seems to the reviewer that that "putting of an end to the drive" is not such a simple thing as it sounds. What really occurs when a drive, as defined by Miss Washburn (see proposition No. 1), is ended is to substitute another physiological state for the physiological state which is the drive. Now, if that is the case, what have we when we examine again proposition No. 5? We have just what Miss Washburn has been at such pains to explain, *i.e.*, "How can the results of an action increase or diminish the tendency to perform whole series of movements that preceded the action in time?" That is, we have a physiological state which was not present at the time the movements were made, but occurred afterwards, causing the elimination of some of those movements. Consequently, we have moved neither forward nor backward so far as the explanation of elimination of errors is concerned.

In conclusion, the reviewer does not wish to seem too unsympathetic in his treatment of this book, for, to be so, would be to confess inexcusable ignorance of the great difficulties which immediately surround one who attempts such a work. The volume of material to be dealt with is enormous; the task of organization is stupendous; the critical analysis of the mass of experimental data taxes the clearest and most logical mind. Miss Washburn is to be congratulated upon the authorship of this volume. She has done an excellent job. No student of animal life should be without access to a copy of *The Animal Mind*.

W. T. HERON

University of Minnesota

BOOKS RECEIVED

BOOK, WILLIAM F., *Learning How to Study and Work Effectively*. Boston: Ginn, 1926. Pp. xvii+475.

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ROUHIER, A., *Le Peyotl. La Plante qui fait les yeux émerveillés*. Paris: Doin, 1927. Pp. xii+371.

NOTES AND NEWS

At the meeting of the American Psychological Association held at the University of Pennsylvania, Philadelphia, Pa., on December 28-29, 1926, the following elections were announced: President, H. L. Hollingworth, Barnard College, Columbia University; Council of Directors, J. Peterson, Peabody College for Teachers and W. S. Miles, Stanford University; Representatives on the National Research Council, H. S. Langfeld, Princeton University, and K. S. Lashley, Institute for Juvenile Research, Chicago, Ill.; Representative to the Social Science Research Council, K. Dunlap, Johns Hopkins University.

At the meeting of the American Association for the Advancement of Science held in Philadelphia the following Vice-Presidents were elected: For Section I (Psychology), Knight Dunlap, Johns Hopkins University; for Section Q (Education), Arthur I. Gates, Teachers College, Columbia University.

THE next meeting of the American Psychological Association will be held at Ohio State University, Columbus, Ohio, December 28-30, 1927.

PROFESSOR M. J. ZIGLER, of Wellesley College, will teach at the University of California, Southern Branch, in the summer session of 1927.

By a recent ruling of the faculty of the College of Liberal Arts at the University of North Carolina, the elementary courses in psychology have been included among the alternatives that may be offered in satisfaction of the sophomore requirements in natural science for all students.

At the annual meeting of the Board of Directors of the Psychological Corporation, held on December 2, 1926, the following officers were elected: President, W. V. Bingham; First Vice-President, W. D. Scott; Second Vice-President, L. M. Terman; Secretary and Treasurer, P. S. Achilles, and Assistant Secretary, E. O. Bregman.

MISS ELEONORE WUNDT writes that she would like to receive letters addressed by her distinguished father, Professor Wilhelm Wundt, to American psychologists, for use in the biography that she is preparing. Any such letters will be copied by her and promptly returned. They should be sent to Bismarckstrasse, 31, III, Jena, Germany.

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